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COMPETITION IN CHILDREN: AN EXPERIMENTAL STUDY

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The literature on competition is plentiful. The importance of this human inclination is everywhere acknowledged, and it is everywhere exalted as one of the prime motive powers in the accomplishment of the world's work.[1] The *nature* of competition is perhaps the subject of an equal amount of discussion, and here agreement is not so universal. There is a tendency among psychologists to try to determine whether competition is a primary instinct, or merely a human tendency that can be developed by nurture and directed by social relationships. The argument depends much on terminology and the terms vary with schools of thought. A decision between the contestants has no especial significance for this investigation. For the purpose of clarity of thought, however, it seems advisable to take a definite stand in the matter of definition. So, throughout the following pages, competition will be understood to mean that human tendency which consists of *a desire to excel, an impulse to do better* than our rivals.

Some studies in rivalry have already been made, although the field has not been extensively investigated. Our present research is in the field of child psychology, and we shall report particularly our findings on work in the field of experimentation with children. The experiments of Moede are those which first come to mind.[2] His comments on rivalry are based on a series of experiments in which a child tried to attain a good result on a dynamometer. The child tried with all his might; but when a second child was brought into the situation, his power in-

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[1]This work was carried out under Dr. Charlotte Bühler, Director of Research of the Psychological Institute of Vienna. The author wishes to thank her, and her assistant, Dr. Hildegard Hetzer, for their valuable suggestions.

[2]W. Moede, *Experimentelle Massenpsychologie*, 1920, 1-73.

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creased and he did better. Moede proceeds to enlarge on the effect of the contest on the contestant and on the group.

In her study of the popular or folk plays of children, Hetzer discusses the competitive plays of children. She found that before the age of 3 yr. rivalry or competitive plays were absent. Between the ages of 3 and 6 yr., 8% of all the child's play is competitive. She says in part:

> In the plays of early childhood blind chance and the age of those playing together decide the social position of the individual child. The distribution of the various 'offices' of the game is determined, for the most part, by the rules of the game. Individuals have no opportunity to change their social position by personal merit. Contests of this type are found first in the plays of later childhood, in the group plays of comrades of equal age. After the sixth year of life the contest plays take on an ever widening aspect. Bodily skill above all, but mental skill as well, help to win. The ability characteristic of the ideal leader of certain age-groups is preëminent here. With boys at the age of puberty, bodily power, pluck, and courage come first; with girls success depends on drawing, singing, roller skating. Before the tenth year this sex difference is lacking. In early childhood 'rivalry plays,' and in later childhood 'neutral companion plays,' move into the background.[3]

A very careful and exact study of despotism and rivalry in babies has been made by Charlotte Bühler.[4] It is concerned with the behavior of children from 4 to 22 mo. of age, and is a detailed study of how children of this age seek to dominate the situation and thereby display their despotism. In this situation two children of varying age relationships were placed together and a toy given to one of them. Bühler says that if you expect an immediate battle for the toy you will be disappointed; "the child who does not receive the plaything is satisfied to look on, to see what the other child does with the toy, for at this time the looking gives him as much, or even more, pleasure than grasping. Also, when an older child takes away a plaything from the younger, the younger is so completely occupied with looking that ceasing to have and grasp the thing does not yet cause him to suffer and seems in no way to injure him."[5] She continues, "Despotism between children springs from other causes and events than prevail between the child and the grown-up. The play situation between two children coming together for the first time is almost completely neutral. When there is only 'looking contact,' one can observe the neutrality of the relationship. It is with grasping that the 'collision' begins."[6]

She traces the development of despotism in these youngsters, the attempt on the part of one child to impose his superiority on the other by the strength of his activity. Her findings are as follows: (1) The child of the first half-year does not yet enter into a relationship of despotism and rivalry. At this age rivalry is incidental to the object and despotism of the older child. (2) In the second half-year rivalry and despotism are found vigorously developed. (3) Age relationship plays a very definite part in the stages of development of rivalry and despotism. Only children of approximately the same age (maximum difference 2½ mo.) rival each other, and the older child is almost always the despot.

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[3] H. Hetzer, Das volkstümliche Kinderspiel, *Wiener Arbeit. f. päd. Psychol.*, 6, 1927, 75.

[4] C. Bühler, H. Hetzer, and B. Tudor-Hart, *Soziologische und psychologische Studien über das erste Lebenjahr*, 1927, 1-102.

[5] *Ibid.*, 55. [6] *Ibid.*, 56.

American psychologists likewise have interested themselves in the subject of rivalry, and their investigations throw light also on this hitherto obscure but important subject. Hurlock investigated the effect of incentive on performance, and reports that children do better after either praise or reproof than after mere repetition.[7] Ross has shown that motivation is greater for bright children than for dull ones.[8] Both studies corroborate the empirical psychological statement that competition is an energizing and stimulating human impulse under whose influence the best work is often accomplished.

To sum up the preceding opinions: they seem to indicate that competition is an extremely important human impulse, playing a vital part in life's work; that competition directly influences and vitalizes the quality of this work; that competition and pugnacity or despotism are very closely related, though not identical; and that pugnacity appears to precede competition.

Object

The object of the present investigation was to study a competitive situation, and to determine: (a) whether, in a given situation, competition always exists; (b) when, in that situation, the tendency first occurs; (c) whether, if present, it exists always in the same degree; (d) how it manifests itself; and (e) what causes it to vary.

This report falls into two parts: (I) An evaluation of the degree of competition displayed in the child's performance when he uses concrete material in a specific situation. (II) An analysis of the factors causing the variations in the manifestation of competition. This analysis includes a discussion of competition as an inclination developing normally and in definite relation to certain laws of growth; a consideration of the part education plays in the growth of competition in childhood, and an evaluation of the part that personality and individual differences play in the exhibition of competition.

Method and Procedure

The greater part of this experiment was carried out in the department of Kleinkinder of the Kinderübernahmsstelle in Vienna, although children in two other kindergartens were also observed. Children from the age of 2–7 yr. were included. The experimenter entered the room where the children ordinarily lived, and choosing according to her needs the ones with which she wished to work, asked them if they would like to build. In practically 100% of the cases the answer was, "Yes." If not, that child was not urged against his will to go with *E*. Two children were then taken into a smaller room, where they would be less disturbed by others. Here they found a small improvised table, a rather wobbly one, and on the table a pile of building blocks. These blocks were smaller and more varied in size and in shape than the ones with which they were familiar.

[7] E. B. Hurlock, The effect of incentives upon the constancy of the I.Q., *Ped. Sem.*, 32, 1925, 422-434.

[8] C. C. Ross, An experiment in motivation, *J. Educ. Psychol.*, 18, 1927, 337-346.

The material offered the interest and challenge of novelty and of a little more difficulty than that of the material already familiar. On the other hand, it was of a sort with which any child could do something, even if only to handle it. The factor of uneasiness with *E* was absent because of her frequent previous contact with the children. Even the very little children always came eagerly to build.

The time, the place, the material, and the *E* were constant in this experiment. We were not, of course, constantly dealing with the same children from week to week, since they remained in the institution only about three weeks. From one point of view, however, the *type* of child was 'constant' and represented in most cases the milieu often designated as proletarian. But the children's parentage comprised every possible mixture of type and quality, from the point of view both of heredity and of environment. Most of the children came from poor homes, but a cross-section of their homes represents exceedingly well a cross-section of society in general.

The children, therefore, of varying ages and of varied home background,were brought two at a time into a small but very pleasant room, and seated on their own little stools opposite each other at a table with building blocks. The blocks were piled in the middle of the table and were not divided between the children; each child could reach out and take as many as he needed or wanted. For the most part they were glad to play with the blocks or build with them, which suited *E*'s purpose very well. Without instructions beyond being encouraged to go ahead and make something, they were permitted to build freely at will. The construction was considered finished when either child said, "I have finished," or when he had used up all the stones. When both children had finished they were asked to come and stand by *E*'s side and to look carefully at each other's construction. Then they were asked, "Which is prettier?" This first construction, carried out without an assignment, and inclusive of the child's answer to the question, "Whose is prettier?" was considered the first part of the experiment.

After they had answered the question the children were permitted to build again, but this time they were given an assignment. *E* pushed all the blocks into the center of the table again, and looking at both children said, "I would like to see who can build prettier this time?" or "Would you like to see who can build prettier?" In some cases the children built a third time, the assignment then being, "Let me see who can build bigger."

The activities, the remarks, the gestures, the whole mood of the situation, were recorded in the protocols as completely as possible. The name, sex, and age of each child were registered and a record kept of all data concerned with the experiment. In the 2–3 yr.-old group there were 2 boys and 6 girls; in the 3–4 yr.-old group 8 boys and 7 girls; in the 4–5 yr.-old group 5 boys and 6 girls; in the 5–6 yr.-old group 11 boys and 9 girls, and in the 6–7 yr. old group 7 boys and 4 girls.

I. Behavior in Competition

The constructive situation of the experiments has two basic elements or components out of which the problem of competition arises. The child cannot escape from some sort of contact with either the other child or the material. He has been placed in a social situation wherein he builds with another child, and he has been given blocks

as material with which to work. Competition is concerned with both, because out of the child's relationship toward the other child develops the inclination we are here calling competition. This statement may be reversed: the child indicates the degree of competition which he experiences by the way he behaves toward the material and toward the other child. In this section of the paper, therefore, we shall devote ourselves to an examination of the child's behavior in these two respects, and from this examination we hope to gain criteria by which we may evaluate the degree of competition the child is exhibiting.

(*A*) *Relation to material.* The child's relation to the material is not constant; his reactions change not only as the experiment is repeated or varied but also during the progress of any one building. He comes to the situation for the first time, fresh, and his behavior at the very first contact with the material seems to convey a definite significance. To keep this reaction unadulterated *E* gave him no directions. She simply asked him, "Would you like to build with some blocks?" When he answered "Yes," she brought the two children into the room and into the situation already described.

(*a*) *Early behavior.* What did the child do when he sat down? At least one of several quite different things: he (*1*) merely *played* with the blocks, or (*2*) did *nothing* with them, or (*3*) began at once to *build.*

(*1*) *Played.* When first confronted by the situation, some children began immediately to play with the blocks. This is an exceedingly characteristic and unmistakable type of behavior. Such a child did not hesitate for a moment. He was scarcely seated when he touched the blocks, drew them toward himself, pushed them away again, lifted some up, let them drop, twirled them around in little circles, etc. These movements were mostly arm and shoulder movements, at times involving the hand, but almost never employing the smaller muscles of the wrist or fingers. The child's attention in these cases was given to the material not as something with which to work, but merely as material with which to do something, and from the constructive standpoint his movements were undirected and non-specific. In 50% of the cases this mere manipulating continued throughout the experiment; in the other half, the child passed from such undirected functioning into a more constructive handling which at times closely resembled building. But even when the child did eventually build, his behavior could be characterized by the word 'uncertain.' Not 1% of the children at this stage were found to give these uncertain constructions a name.

(*2*) *Did nothing.* To the group just described belong the children between the ages of 2–3 yr. There seems to follow in the slightly older child (3–4) a period wherein he does not attack the material at the very start, but delays handling the stones and gives his attention to his surroundings. He looks around the room,

makes remarks about things or persons in the room, or else merely sits still and does nothing. He appears not to see the blocks as a special object of attention, and does not begin to touch the stones until the other child has begun to build. Take for example the following protocol:

Boy, H.E., age 3·3. While companion builds steadily, H.E. looks around, makes remarks on the toys in the room. Hears some water running and turns toward it. Listens to it, keeping his head turned in that direction. Knocks over other child's building (unintentional, occurred through movement). Coughs. Talks about stones. Yet when asked whose is prettier he said, "Mine."

(*3*) *Built.* In the third type of behavior the child goes immediately to the blocks and handles them constructively, making something out of them. These cases were very numerous and appeared to increase with chronological age.

Table I and Fig. 1 show the relation between age and the frequency of the three types of behavior just described. It is not hard

TABLE I

RELATION BETWEEN AGE AND TYPE OF BEHAVIOR

Age-Group (yr.)	Percentage who		
	do nothing	play	build
2–3	0.0	62.5	37.5
3–4	31.6	21.5	46.9
4–5	28.7	14.3	67.0
5–6	0.0	22.7	78.3
6–7	10.0	0.0	90.0

to understand why the child of 2 or nearly 3 years of age begins at once to manipulate the stones. The infant made his first random and undirected movements without any relation to an outside object. As the child develops and comes into contact with material, this progression from non-specific to specific reaction is repeated with each new material. Consequently the young child to whom these building stones are a new material, and whose building experience is very limited, makes toward that material undirected and non-specific movements. The 3-yr.-old has discovered that manipulation has an end-result, and that out of the material he can make 'a thing.' This interest in the material leads the child to see what he can do with it and he builds. The figures show a very interesting rise in the percentage of children whose attention to building grows, starting with 37.5% in the child of 2 yr. and reaching 90% in the 7-yr.-old. When, however, the child of 3 yr. is arriving at a phase of development wherein he is interested in effort with the material, he is also approaching the social age. He appears more interested in his

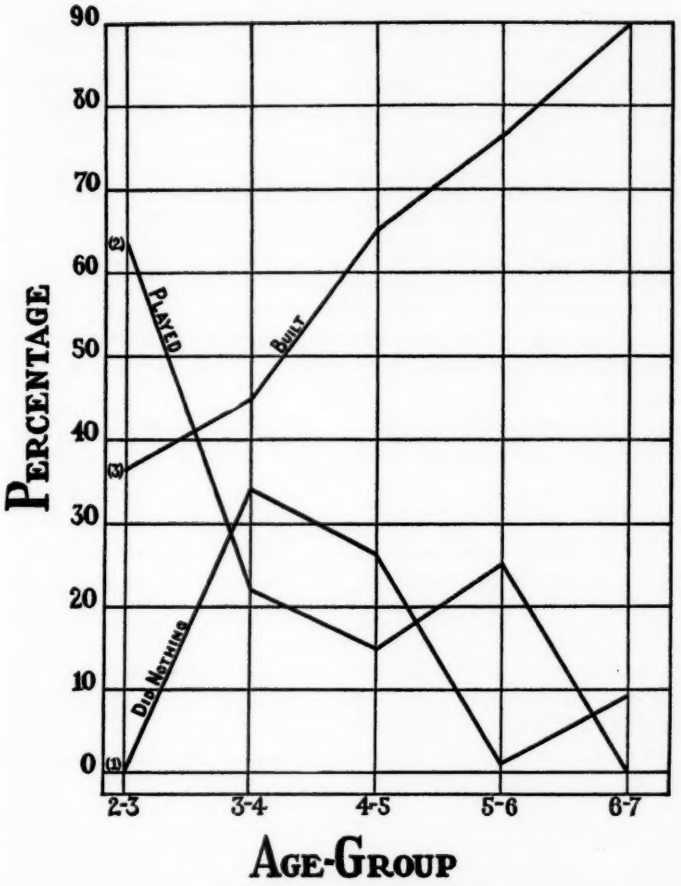


FIG. 1. EARLY BEHAVIOR WITH THE BUILDING MATERIAL

surroundings, in the other child, in other people, in the other things in the room, than in functioning through the blocks alone. The percentage of children who sit before the blocks and do nothing with them rises from 0% in the 2-yr.-old to 31.6% in the 3-yr.-old. The percentage falls when the child reaches the age of 5 yr. and is apparently familiar with the material. Then he wishes to begin work at once with it.

The child's behavior at the very start, then, is integral with his understanding of the problem. The child of 2 yr. does not see the blocks as material; he sees them as things with which to function. The child of 3–5 yr. already recognizes the material as stuff for work, but his interest in the other child is perhaps the stronger influence. He does definitely recognize the blocks as a creative medium, and passes rapidly from a functioning to a working behavior, all the time being even more aware of the other child in the situation. Thus, as has been said, at the very beginning of the problem the child's behavior falls into three types: functioning with the material; doing nothing with the material (at the start); working with the material. The correlation between the child's *mastery of the material* and his recognition of the *idea of excelling* will be discussed later.

(*b*) *Behavior during building.* The child's behavior with the material does not, however, confine itself to what he does at the beginning of the experiment. The experiment continues, and by a study of what he does later in its course we can obtain some further idea of the progress of his mastery of the material and the correlation between mastery and competition. The casual observer might say that surely if the child builds at all he merely uses the material. But close observation distinguished some interesting variations in the way he handles the material. The reference is not to the skill with which the child uses the blocks but rather to the manner in which he *reaches out* for them. Muscular behavior seemed to have a direct and rather close relation with the feeling-tone of competition, as Table II indicates.

(*1*) *Used as needed.* There were a large number of children who simply used as much of the material as they needed, reaching out for blocks one or two at a time, either taking those nearest at hand or choosing the ones which seemed best fitted to the construction they were making. This kind of reaching is itself varied, of course, and depends on the child's development and skill; but it is mentioned here as one type of behavior towards the stones, in which the child "using the material for his needs" is responding to the *need for stones* and not to the *social situation* or to the impulse of competition. He is fulfilling his needs in the interest of the work.

But this type of action does not always obtain. Without attempting at this point to analyze the cause of these variations, but merely to describe them, it may be said that while many children simply used the material as they needed it, others very definitely exhibited one or two other kinds of action: 'grabbing,' or 'giving.'

(*2*) *Grabbed.* The child who 'grabbed' reached and took what lay on the other child's side of the table. There can be no doubt that such a child was exhibiting something different from a need for material. It was indeed a need for certain material, but it was a need attended both by a disregard for what belonged to the other child and at the same time by an anxious regard for what the other child would say in response. This high-handed disregard was demonstrated by such a startling act as the child's standing up, reaching right into the middle of the other child's construction and taking what he wanted. At other times the child took stones surreptitiously (say from a little pile the second child had gathered ready for use), and watched all the time to see if he was observed. There were cases where he took many stones and hid them slyly in his lap, and also where the recognition of grabbing led to out-and-out quarreling. The close relationship between the competitive impulse and the instinct of pugnacity was dramatically clear.

TABLE II

BEHAVIOR WITH BLOCKS DURING BUILDING

Age-Group (yr.)	Percentage who		
	grabbed	gave	used as needed
2–3	0.0	16.6	83.4
3–4	0.0	5.2	94.7
4–5	30.7	0.0	69.3
5–6	36.4	13.6	50.0
6–7	27.3	18.2	54.5

(*3*) *Gave.* A more subtle piece of behavior and one a little more difficult to analyze was the 'giving' of stones. At times this giving was in response to a request for stones. But oftener it was a spontaneous giving, apparently divisible into two types. One was a mere social giving; another child was in the situation and the child thrust stones toward him. The second was in response to a recognition of the other child's need of the stones, a recognition which is involved in an understanding of the situation. At times the child offering the stones would observe the second child searching or looking about. Then he would say, "Do you need this?", handing him at the same time a stone. Generous, however, as this act appears to be, it is closely connected with the child's recognition of the problem, and is bound up with the impulse of competition. For while the 2-yr.-old stage does show in the following table a greater number of children giving stones than appears from 3 to 4, it must be remembered that this giving is largely a muscular movement, just a shoving of the stones toward the other child. In the next age-group, when the child shows many other signs of social interest, that is, looks more at the other child, talks to him, etc., he does not yet give him stones. The reason for this will be discussed in the next section of this paper. Let it suffice to say here that while he is interested in the other child he does not yet recognize him as part of the building situation.

There is one other comment to be made on the way the child behaves with the material. The child of 2 yr. at no time asks for stones; while the child of 6 does so with a very clear indication of conscious action. This very simple observation has, however, a deeper significance, and gives a general view of the whole development. The child passes from a mere stimulus-response reaction and equally non-

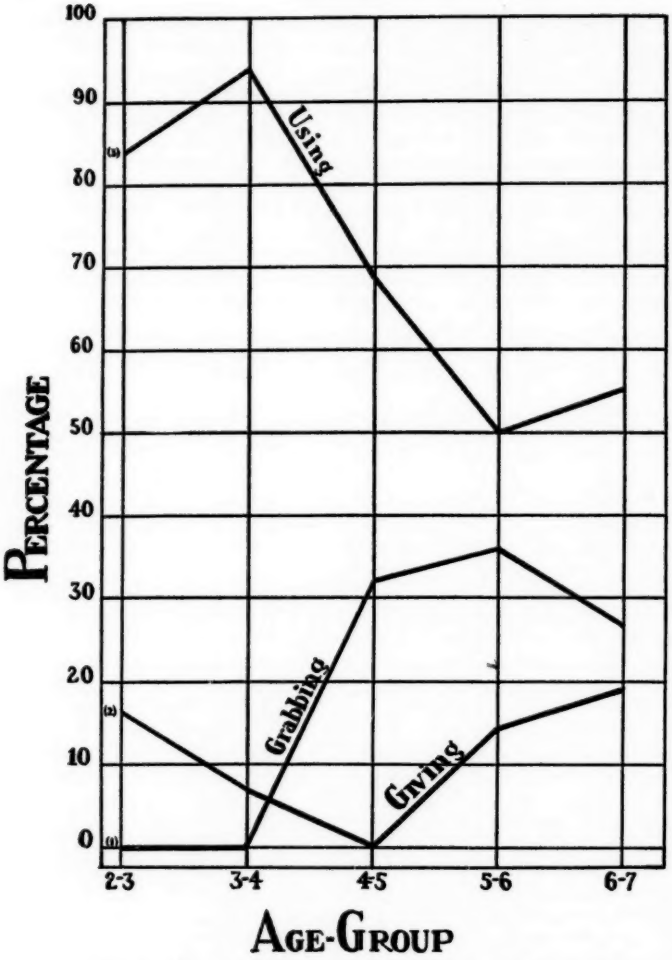


FIG. 2. BEHAVIOR WITH THE MATERIAL DURING BUILDING

thoughtful giving through the steps of (a) neither giving nor taking. (b) then a decided grabbing, (c) finally a stage where he does take what he wants, but also asks for what he needs, and thus exhibits fully an understanding of the problem.

(*c*) *Interest in work.* We may include as indicative of the degree of competition the child's attitude toward his own work. This is, of course, the most definite of

the indices, but at the same time the most subtle. For while it must be described in terms as concrete and definite as possible, still it involves what is called by some psychologists 'a feeling-tone.' That is, cases frequently occurred where *E* 'felt' competition more keenly than she could describe it. She was finally able, however, to isolate and organize some of the phenomena that suggested 'competition.' These phenomena were (*1*) degree of attention, (*2*) physical signs of interest, (*3*) remarks about own and other's work, and (*4*) child's judgment concerning the work.

(*1*) *Attention.* It seems logical to regard the degree of the child's attention as a measure of his interest. In order to determine, therefore, whether a child was exhibiting interest in the building process, we used the quality of his attention as one means of measurement. The child was said to be attentive if he directed himself to the blocks and either manipulated them or built with them throughout

TABLE III

PERCENTAGE OF CHILDREN SHOWING POSITIVE ATTENTION TO THE BLOCKS

	Age-Group				
	2–3	3–4	4–5	5–6	6–7
Attentive	75	40	71.5	81	90
Inattentive	25	60	28.5	19	10

the experiment. Such a child attacked the problem with eagerness and at times even with force, and remained at his task until he had completed it. Sometimes he gave passing attention to extraneous sounds or intruding persons, but such attention was momentary and relatively superficial. His conversation, too, concerned itself with the work in hand and not with his social life or other interests. That is, of course, an extreme picture; there were many children who could not measure up to this high standard and yet gave the observer the impression that they were interested. I shall give here two protocols to make my meaning clear:

An inattentive child.

Girl, E.J., age 4·0. Does not build. Does not play with the stones. Gazes around the room. Looks at Fritz. Wiggles in her seat, looks out into the hall. *E* asks her, "Can you build?" She answered in a slow sleepy voice, "Oh, yes!" *E*, "What are you building?" Answer, "A house."

The other child pointed to E.J.'s stones and said, "That isn't a house." She answered, "Yes, that is a house." At the question, "Whose is prettier," she did not answer, merely pointed to the other child's construction.

An attentive child.

Boy, H.H., age 3·6. He says: "There! I am making a train." Talks continually to himself as he works, but keeps on working and does not deviate at all from the building. "So, now I lay something here. Oh, Oh!" Puts a stone on the construction. Goes on building without looking up. Seems to have his train finished. Says, "Express train. An express train. Well! Here's a bridge." (Points to it.) Talks to himself as he builds, "A boat is sailing under the bridge." *E* asks, "Are you finished?" Answer, "Not yet." In a few moments he says, "Now I am finished!" Gets up and comes to side of *E*. At question, "Whose is prettier?" he looks at both and finally points to his own.

Let us regard this from the quantitative standpoint and see how the percentage of attentive responses varies with the age-groups of the children.

It can be readily seen from Table III that the child of 2–3 yr. of age was attentive to the blocks. It should be noted that he did not build with the blocks, but rather manipulated them and busied himself with them. He however gave his attention to the blocks. The slightly older child, the child from 3–4, was distracted by outside sounds and sights. He gave attention readily to any passing disturbance. From then on attention increases until the child of 6–7 exhibits 90% interest. The existence of even this 10% of disinterest was doubtful, and must be considered again when we look for the more personal causes of variation.

(*2*) *Physical signs.* A little easier to observe were those physical signs by which the child seemed to show unmistakably when he was especially interested. We shall try to show the relation between the physical manifestations and the feeling-tone accompanying competition. As soon as a child became aware of the real meaning of the situation, these physical signs showed themselves very clearly and sometimes a child who when building for the first time (that is, without an assignment) had been quiet and relaxed, became restless and intense under the excitement of competition. Sometimes rather slow movements changed into rapid ones, and relaxed muscles stiffened. The child experiencing the competitive impulse sat more erect in his chair, sometimes on the edge of his seat. He reached out with short, jerky, motions. Color increased and often the eyes brightened, although quite often the lids came down over the eyes, either to increase the surreptitiousness of his glance toward the other child or because of the tenseness of the eye-lid muscles. Children exhibiting this competitive impulse at times rose out of their seats—two actually exchanged seats.

Looking through the protocols I find many phrases such as the following:

[Protocol 6] Laughs, puts tongue between lips. [47] Body tense. Jumps up and down. Claps hands, sings. [55] Every added stone brings a glow to his face. [56] Child cries and says his building is "horrid." [79] Increases speed. Doesn't stop till all the stones are used up. [80] Makes many restless movements of the body. [112] Can scarcely wait to begin. Eyes sparkle. [124] Jumps up and down. Begins to get excited. Talks very loud.

Thus in many physical ways the child proclaims to the onlooker that he is not passively manipulating the blocks, nor even merely using them as a material means to fulfill his building interests, but that he is aware of a social situation and has a desire to excel his partner, a will to succeed. Not all of these physical movements need be consciously directed toward the goal, but one does feel that a child acting in this manner is bending all his energies to the purpose of surpassing a competitor.

(*3*) *Child's remarks.* Many children showed by their remarks that they were quite aware of the problem, that they understood the situation, and that, further, they wished to excel.

A few examples must suffice. For the sake of clarity I have organized them into groups, but they all seem to have one underlying theme, self-exaltation, with all that this implies:

(a) Remarks about the need of certain materials so as to continue the work.

"I need two big stones."—"I want a longer one."—"Have you some more of these very little stones?"—"You have so many, and I have so few!"—"I need many, many, many."

(b) Remarks about the sort of thing they are making.

"I am making a train."—"I like to make only towers."—"I can also make a garden."—"So! Here is a window."

(c) Remarks on their own work showing pride or desire to excel.

"A big house!"—"Look! Isn't mine pretty?"—"Look at me!"—"I'm not finished. I must work very hard."—"Bigger! Bigger! bigger! bigger!"—"My stones don't fall over, I give attention."

(d) Extraneous remarks reflecting glory on themselves.

"My mother is going to get me when she has money."

Table IV shows the percentages in which these different classes of remarks occurred.

TABLE IV

PERCENTAGE OF REMARKS ABOUT OWN WORK

	Age-Group				
	2–3	3–4	4–5	5–6	6–7
Positive	0	10	26.6	43.4	70
Negative	0	0	0.0	0.0	10
None	100	90	74.4	56.6	20

An analysis of the causes of these variations will be made later. The development proceeds, of course, from the absolutely uncritical attitude of the child of 2 yrs. of age to the much more critical attitude of the child of 7. The 7-yr.-old includes some negative remarks about his own work.

The tendency to make remarks has a direct bearing on the display of competition. The child who says nothing at all about himself, does not understand the situation; he does not see that the situation is a competitive one. He shows what he feels by what he says. The child who exclaims, "Sister, look, look at mine. How nice it is!" gives an unquestionable picture of a child awakening to a desire to excel. The child who says nothing probably feels nothing, unless, of course, he shows feeling by some other means.

(4) *Child's judgment.* In the child's judgment of his own work, we might expect to find another means of studying the competitive interest. *E* found it very hard, however, to understand these replies, for it seems probable that the assertive ego in the child is the strongest feeling he has and makes it difficult for him really to compare his work with that of another child. Each time after the children had built they were asked to stand for a moment at *E*'s side and to look thoughtfully at each other's work. Then they were asked, "Whose is prettier?"; or, "Whose is bigger?" The answers were by no means always logical, and seemed to be actuated by almost any motive except a decision according to worth. The answer of the 2-yr.-old child was interesting. In 58.3% of the cases he did not understand the question, and either did not answer at all or repeated the last word of *E*'s sentence, *e.g.*, *E*, "Which is prettier?" Child, "Prettier." *E*, "Who made the best building?" Child, "Building." Most of the remainder of the 2-yr.-olds answered, "Mine," without apparently thinking about the matter or weighing their answer. So far as *E* was able to analyze the situation, there was no correlation in this age-group between the child's building ability and his answer.

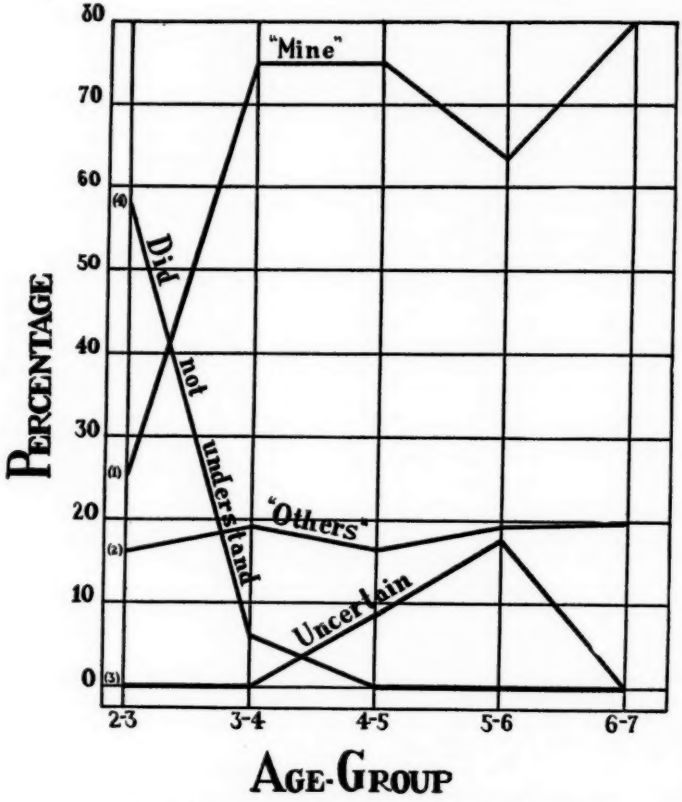


FIG. 3. DISTRIBUTION OF VARIOUS TYPES OF REMARKS

	Percentage Every Age-Group				
	2-3	3-4	4-5	5-6	6-7
"Mine"	25.7	75	75	63.4	80
Did not understand	58.3	6.2	0	0	0
"Other's"	16	18.8	16.7	19	20
Uncertain	0	0	8.3	17.6	0

Likely the answer depends on his comprehension of the words used. If the child of this age understands the question, the answer, without hesitation, is "Mine." There was one exception to this generalization; this was the case of a boy who was not particularly interested or attentive, and who simply pointed to the other child's construction. In the next age-group (3-4 yr.) there were still some children (6.2%) who did not understand the question. Nearly all the children of this group answered, "Mine," without any hesitation. It may seem a contradiction

that children who are at the most social age should prefer their own work without giving the work of the other child a thought. But a study of the situation reminds us that this social relationship has, as yet, nothing to do with the problem in hand. So he talks to the other child, looks at him, is thoroughly aware of him, yet if you ask him, "Whose building is prettier?," he says, "Mine."

There were a few isolated cases where the child replied by pointing to the other child. In two cases this occurred in children approaching 4 yr. of age (3·9 and 3·11, respectively); these were children who were building with a sister, and moreover, an older sister. In a third case the second child was older. In *no* case did a child consider the work of a younger child better than his own.

With the 4-5 yr.-old child the kind of answer remains much the same. At this age-level, 75% of the children consider their own work the better; 8.3% are uncertain, and 16.7% consider the other child's better. The percentage answering, "Mine," is higher in the next age group. Of the children between 5 and 6 yr. of age, 81% finally considered their own better, although of these 17.6% were uncertain just what to say. 19% of this age group considered the other child's prettier. The still older child (6-7 yr.) displayed less hesitation; 80% said, "Mine," without hesitation, and 20% indicated the other child's work.

Fig. 3 shows the distribution of the various types of remarks among the different age-groups.

It may be seen first, that the child of 2 yr. either does not understand the problem, or answers, "Mine." Secondly, that understanding of the building situation rapidly increases, and by the age of 4 yr. every child appears to know what is meant by 'prettier' or 'bigger.' Thirdly, that at no age-group in this experiment did the child seem to be greatly interested in the other child's building. We have already observed that he was much interested in the other child, but when it was a question of construction, his own work demanded his attention. Fourthly, that there is a rise in the percentage of children who consider their own work better.

These figures do not tell the whole tale, however, and we cannot leave the topic of the child's attitude without giving some attention to the differences contained within that answer, "Mine." The numbers do show a high percentage of children who answer this way. But we wish to call attention to the difference in the quality of this "Mine." Note the difference in the feeling one gets as to *thinking* on the part of one child and *no thinking* on the other:

Girl, C.K., age 2·6. Pulls many stones toward herself. Talks constantly but does not build. Looks around. Plays with stones. Talks to stones. Clean wash comes in. She remarks on the wash. Plays with stones. Finally builds a little. Question is put, "Which is prettier?" Says without hesitating or thinking, "Mine."

C.K. gives no evidence of understanding the problem as a competitive one. She merely gives an answer. The child in the following protocol cannot build either, but shows both by her remarks and by her manner of answering that she would like to excel.

Girl, R., age 3·6. (with K., age 6·0.). Very diffident. Does not seem to know how to begin. *E* helps her to build as an encouragement. She does not yet build. *E* says, hoping to stimulate her, "I would like to see who can build better." She still does not build. *E* says, "R. and I will build." R. says, "I will make a train." Other child says, "I can also make a train." R. says sneeringly, "That isn't a train." *E* asks of both, "Whose is prettier?" R. points to construction which *E* built, and says, "Mine."

Still more indicative of the competitive impulse well developed is the following protocol:

Boy, E.A., age 4·6 (with H.G., age 3·11). *E*, "Would you like to build?" E.A., "Yes." Builds with speed and interest. With gusto says, "I am soon finished." Points to H.G., "He isn't finished yet." When both children have finished they stand by *E*. *E*, pointing to H.G.'s house, "Isn't that pretty?" E.A.,: "Yes, but once he hit me with a stone!" *E*: "Whose is prettier?" E.A., quickly, "Mine."

It seems from the foregoing examples that the answer "mine" does not always have the same qualitative value. Hence, the percentage of answers to that effect does not indicate the degree of competitive impulse. All that we can say definitely from the above graph is that verbal behavior is clearly correlated with the other indications of competition.

(*B*) *Relation to the other child.* We have thus far discussed the child's relation to the material; now we shall consider the responses to the other child.

(*1*) *Watches.* There were cases in which the child did not appear to observe the other child at all. This was true of the very young child who came to the table and was barely seated before he began to lay hands on the material. He appeared to notice nothing except that there were stones which he could manipulate. For example:

Girl, C.K., age 2·6. She is brought to the room where she finds table, chair and blocks. She seats herself at once in the first chair she comes to, the one nearest the door. Without waiting she begins at once to manipulate the stones. She touches them, shoves them about. Some stones fall; she picks them up. She talks constantly in an indistinct mumbling which cannot be understood. Jigs and jumps about with stones. She bumps up and down in her seat. Pulls some stones toward herself, shoves them away again. Pays no attention to the other child. *E* asks, "What are you making?" No answer. Goes on manipulating the stones. At close *E* asks, "Whose building is prettier?" Answer, "Prettier." *E*: "Who can build the best?" Answer, "Best."

It does not seem too much to say that this little girl was wholly concerned with herself and the blocks. This type of behavior was found in 54.5% of the children between the ages of 2 and 3 yr. This type of behavior, wherein the child is busy exclusively with the material as something with which to play, diminishes, as shown in Table V until the age of 4-5 yr. In 71.4% of the cases at this age-group, the child looks mostly at the other child. Then, as his ability and interest in the work increases, the child again appears to devote his attention more to the blocks than to the other child.

"Looking" can be further clarified. Sometimes the looking appeared to be merely gazing at the other child. At other times 'looking' was really copying, and resulted in definite imitation. The percentage of children who copied was at no time great, and was practically the same throughout the various age-groups. This was a surprising result, although it may be explained by the hypothesis that copying is more a question of type of child than of age.

(*2*) *Speaks to.* But the child does more than look at the other child; he speaks to him and about him. These remarks about the other child, as in the case of remarks about his own work, make a fertile field for research. There was

practically no conversation of the parlor sort, no extraneous remarks. The child seemed to devote himself to the object in hand, whether it was mere manipulating, or whether it was building. One heard occasionally such phrases as, "I'm going home this week," or, "Last Sunday I saw a beautiful castle," but they were exceptional. The child confined himself to the situation. The fewest remarks were made by the youngest children. In fact children at the age of 2 yr. made no remarks at all about the companion-child. Table VI shows the percentage of children in the various age-groups making remarks. The things said were specific and connected with the work at hand. They were, as Table VI also shows, mostly of a negative nature.

TABLE V

PERCENTAGE OF CASES AT EVERY AGE-GROUP THAT WATCHES AND DOES NOT WATCH OTHER CHILD

	Age-Group				
	2-3	3-4	4-5	5-6	6-7
Watches	41.7	52.6	71.4	46.5	50.0
Does not watch	58.3	47.4	28.6	63.5	50.0

TABLE VI

PERCENTAGE OF CASES AT EVERY AGE-GROUP MAKING AND NOT MAKING REMARKS, TOGETHER WITH THE PERCENTAGE OF CASES MAKING POSITIVE AND NEGATIVE REMARKS

	Age-Group				
	2-3	3-4	4-5	5-6	6-7
No remarks	100	90.0	62.5	59.1	60.0
Remarks	0	10.0	37.5	40.9	40.0
Positive remarks	0	0	0	4.5	0
Negative remarks	0	10.0	31.3	9.1	20.0

The child's remarks were at times exceedingly curt. He showed, clearly, that he thought better of himself than of the other child, and did not hesitate to say so. Sometimes gestures accompanied these remarks, such as a derogatory sweep of the hand, or a supercilious expression. For example:

Girl, F., age 5·0, and boy, H., age 3·6. Both children have been building very industriously, but H. does not progress very well. He fumbles with the stones. F. says to him: "Na! Can't you even build?" He answers curtly, "Yes." She turns to *E*, throws back her head and laughs, "Ah! He says, 'Yes'," and laughs some more.

Consider also the tone of superiority of this case:

Boy, L., age 4·6, and girl, E.M., age 4·6. L. is slow in building; is apparently not accustomed to blocks. E.M. points to L. and says to *E*, "He there! He can't do anything." Then she points to her own and says, "Isn't that pretty?" Later L. says, "I can build a railroad!" Then E.M. replies in a supercilious manner, "He can't do a thing. *I* can build a railroad."

On a later occasion this same girl was building with another boy (H., age 3·11) who was more capable and built very well. In order to discover what her reaction would be, *E* asked her, pointing to H.'s construction, "Isn't that house pretty?" Whereupon she slowly answered, "Yes." Then suddenly, "*But one day he hit me with a stone!*"

(3) *Gestures.* Looks and words tell us what one child thinks of another. Still more, movements and gestures reveal competitive feelings. Gestures, displayed in the 'despotic' behavior of children, have been exhaustively studied by Charlotte Bühler, who classified those she observed in children, age 4–22 mo., when put in a social relationship with each other. The situation discussed in the present paper is to some extent similar. But this time the children do not have a toy between them, but are working with the same assortment of building stones. Bühler found *Wegnehmen* the earliest gesture of despotism. Although she differentiated sharply between despotism and rivalry, we too find that *Wegnehmen* is one of the first signs of the competitive impulse. Bühler also mentions "hitting," "pushing away," "the gestures of teasing," and the "laugh of triumph." She reports on the other hand that the inferior child permits himself to be given things, to be pleased with things. In the older child who is competing, I can also report all of the preceding gestures. But, in addition, I observed an unusual amount of physical movement, hiding of stones, stealing of stones, singing, and out-and-out fighting. For example:

Girl, H., age 4·8; boy, K., age 4·10. *E* told them that she would like them to try again and see who could build the larger. K. grabbed a number of stones and hid them in his lap. H. objected, "You have taken some of my stones." No answer from K., he merely took another stone from H. Both children very eager.

H., on finishing her building said, "Here's the bigger one." Her body was tense, jumped up and down, clapped hands, sang, "Mine is finished and it is bigger." K. not so tense, worked steadily, and said, "Mine will be still bigger." Took some of H.'s stones from her building. She said, "Don't you take any of mine away." Each considered his own building the larger. (As a matter of fact K.'s was a little larger, but not obviously so.)

This type of behavior is repeated again and again: a little girl leans over and takes a stone right out of another's building; a boy grabs all the stones he can get; another child stands up, apparently for a better physical position; one child snatches stones right out of another's hands; another child piles his stones in little groups and says they "belongs to me"; and so on for many similar cases.

There were also the gestures of intense attention. Sometimes a child would devote himself entirely to his construction. In his case, no matter how much his partner kept on prattling, he went on working. Needless to say, he considered his work better.

As Bühler has suggested, there are also gestures of competition that belong to the benign type. Sometimes when a child gave another a stone, it was not at all an act of disinterested generosity, but rather a display of friendly competition and superior skill. This type of giving, as the following example shows, must not be confused with mere social offering which appeared in the 2½-yr.-old.

Girl, H., age 5·0; boy, E.K., age 5·9. *E* asked them no questions nor gave them commands.

E.K. built industriously and with great interest. H. built, but not quickly. E.K. talked continuously; said to *E*, "that is a table," pointing to the table. H. paid very little attention to E.K., merely said "Hm-hm."

E.K. stood back and looked at his building and said, with pride, "That's done." H. said, "I can't go any further, I need two more stones like those," pointing to a certain size of stones. E.K. went to his building, took out the stones H. needed, and said, handing them to her, "Here, H.; here, H." She took the stones and put them in her building. E.K. asked, "Do you need still more, H?" H. answered, "No."

When finished, both buildings looked almost exactly alike. *E* asked "Which is the prettier?" E.K. said quickly, "Mine is the prettier." H. was slower to respond, finally she pointed to E.K.'s. (These children were very fond of one another.)

II. Analysis of the Factors Causing Variations in Competition

It is the purpose in this section of the paper to analyze those factors which cause variations in the display of competition. Our study of the child's behavior in a competitive situation has shown that competition is not even found in all the children of any age-group. What then causes this variation? Is it a question of physical development? Then strong children would exhibit competition and the weaker ones not. Is it a question of temperament, of training, or of type—or all of these?

TABLE VII

Percentage of Children Exhibiting Competition

Age-Group	Present	Absent	Doubtful
2–3	0.0	89.5	10.5
3–4	42.6	55.6	1.8
4–5	69.2	23.1	7.7
5–6	75.4	15.8	8.8
6–7	86.5	5.4	8.1

To understand the variations in competitive behavior one must look closely at the relation to the material and to the other child, and find the basic principle underlying these forms of behavior. It may be seen from Table VII that the degree of competition increases with increasing age, but that competition is not present in more than half the children until the child is between 4 and 5 yr of age. The growth of competition appears to conform to a regular developmental law which can be analyzed and understood.

Principle 1. The first hypothesis which we offer is that competition develops consistently with the *child's relation to the material.* The child's attitude towards the material is fundamentally altered when he has mastered it. He experiences no sense of competition toward material with which he is unfamiliar or which he has not yet mastered. There was a clear relation between the child's mastery of the material and the degree of competition displayed. At the same time there appeared that progression from functional behavior to social relationship and finally to an interest in the work itself which has been seen repeatedly in experiments at the Kinderübernahmsstelle.

(*a*) *The first stage.* Here no competition could be seen. This phase includes children from the age of 2 to 3 yr., showing at 3 yr. some progression toward the

next phase. Children of this age-group made vague and undirected movements with the material. They were in the stage of manipulation or mere 'functioning.' Of these children 54.5% did not understand the question, did not grab stones, and did not make either positive or negative remarks about their own work. When they did understand, or appeared to understand, the question, in practically 100% of the cases the answer was, "Mine is prettier," without weighing or judging the matter. As the child of this age has no feeling for work, so likewise is the quality of work poor; and competition is lacking.

It is altogether probable that with other material the child would exhibit competition at an earlier age, but it would need to be material which he had already conquered. For with each new material, he must conquer the steps anew. Until he has conquered the material, he cannot be said to work with it; and until he works with the material, the desire to excel in the use of this material does not exist. But of course mastery of the material is not sufficient. He must grasp at the same time the idea of mastery and the idea of excelling. He must know what it means to do better than someone else.

We do not intend to convey the idea that pugnacity is absent in those cases in which the competitive impulse is not yet developed. In fact, in the very smallest children there was evidenced at times some indication of pugnacity. But we mean that the competitive impulse does not always exist contemporaneously with and as a concomitant of pugnacity.

(*b*) *The second stage.* The second phase of development in the competitive situation is one in which the child appears to be interested in discovering the other child. Until this time he has been busy discovering himself, developing his own powers through manipulating the material. As he completes this phase one of two things might happen. Either he might become especially interested in the blocks as building material, or he might become especially aware of the other child in the situation. What does happen is this: his powers are constantly developing in regard to the building, but his marked *interests* are in the social situation, in the other child. Once again, he appears to follow the already accepted path of psychological development: first, self-functioning, then the human contact. This does not mean at all that his skill in the use of the material has not increased, nor his understanding improved; and with both, naturally, some increase in the competitive impulse has appeared. All these go on at the same time. Yet we can say that the child from 3 to 4 yrs. of age is one whose interests lean very much to the social side of the problem.

An examination of the protocols and an assembling of the tables reveals a clear composite picture of the child of this age. It is presented here in Fig. 4.

During the second stage, the child's interest in the blocks is slight compared with his interest in the other child or in the social situation. This does not mean that he pays less attention to his building than the younger child does, but that he gives more attention to other people in the room and to the other child. In this stage he does not, for example, begin to grab stones. He simply uses what is at hand. The younger child does not grab, either; neither one sees the situation yet as a competitive one, and neither feels the need to take stones beyond those which lie close at hand. The younger child makes no remarks about the other child at all. But the child of about 3 yrs. of age begins to talk about the other child. The number of negative remarks increases at first, drops again when the child reaches 4 yr.,

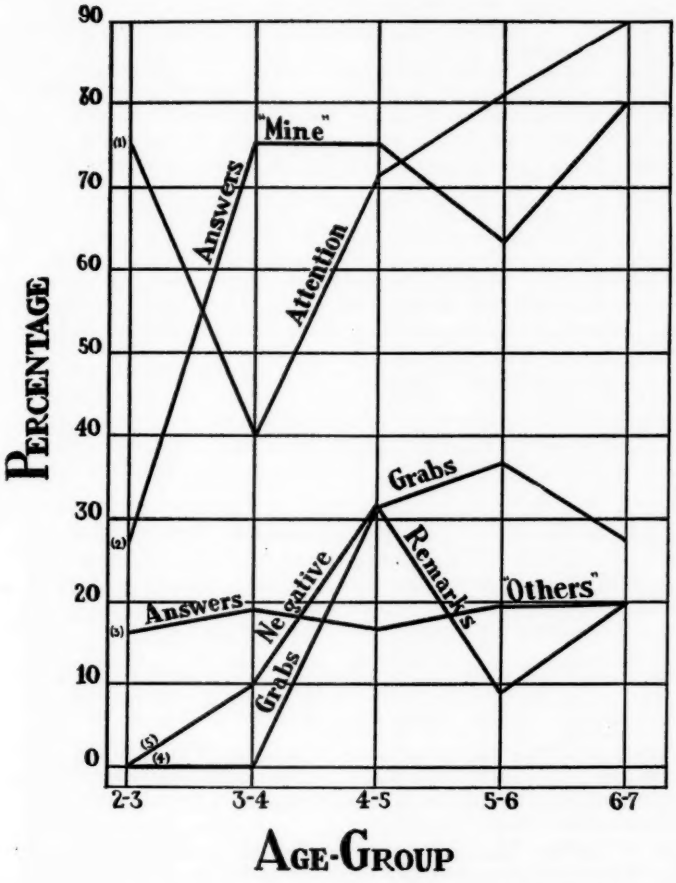


FIG. 4. COMPOSITE PICTURE OF CHILDREN'S RESPONSES

	Percent Every Age-Group				
	2-3	3-4	4-5	5-6	6-7
Attention	75	40	71.5	81	90
Answers "Mine"	25.7	75	75	63.4	80
Answers "Other's"	16	18.8	16.7	19	20
Grabbed	0	0	30.7	36.4	27.3
Negative remarks	0	10	31.3	9.1	20

and is not very great even when the child reaches 5 and has already become definitely aware of the problem. All of which seems to indicate a positive feeling toward the other child.

This is a child who has arrived at a point where he is more interested in the other child than in the material or in building. It may well be said that the other child is, after all, an integral part of the situation, and that a strong interest in him may be the first indication of the competitive aspect of the situation. Our answer is that the 3-yr-old's attitude may be best described as mere 'awareness' of the social contact.

(*c*) *The third stage.* In the third stage the child becomes conscious of the material as stuff for work; he understands the problem in hand, and arrives at a fulfillment of the competitive impulse. Now at this age, 4–6 yr., the child no longer sits and plays with the material, nor does he spend so much time in doing nothing. Instead, he attacks the material at once, as *building-stones*, and begins to work. From 52 to 90% of the children from 4 to 6 yr. of age attack the problem without hesitation. They display their interest in many ways. They begin to grab stones. The percentage of children who give attention rises sharply. The child makes negative, but no positive remarks about the other child, being more interested in what he makes than in the social relationship to the other child. The positive remarks about his own work rise rapidly, from 10% to 30%. He no longer answers "mine" to the question, "Which is prettier?", but seems instead to deliberate.

In other words this attentive, block-grabbing, negative-remark-making, but spontaneous child displays competition. These are the children who quarrel with each other over stones. They simply *have* to excel. These children also exhibit, at times, a patient and yet intense effort to improve their own building, sometimes trying over and over again to improve the construction. There were many instances of this type of effort, of which only a few can be presented.

Boy, R.B., age 6·0. Very eager to build. Says, "I will build a house." Piles stones in a corner. Does not look at C. at all. Sighs, "Yes, what I am making is very pretty. I just need to work a little longer." Grabs many stones. Keeps on working after other child has finished. Says, "A church." Then, "Finished."

Good evidence of the child's interest in the building itself is the way the child in this phase handles the materials. He groups them, putting those of a like shape together, or matches them for size. The following protocol shows this quality of work quite clearly:

Boy, F.J., age 6·0. Begins at once to work. Pushes some stones towards himself and groups them into a pattern. Seeks definite stones. Says, "That is a castle," then holds up a stone. "This one is larger." Holds up the stone and measures it against the other; "No, this one is larger!" Very attentive to his work. Asks *E* for certain stones that he lacks. Goes on building.

The child in this stage is master of the material and struggles to surpass his partner. He tries diligently to excel, and even when he does not succeed, he does not yield to defeat. Consider the case of this boy who was building to see whose would be bigger. When he finished, the other's work was so much bigger that he had to admit it, but could not forbear adding, "But I can build bigger, too!"

(*d*) *The fourth stage.* A still more developed phase in competition when building with blocks appears to exist. The somewhat older child recognizes even

more clearly the meaning of the situation, and his desire to excel becomes tempered by his ability to judge his own work. I found at this stage a very interesting example of a girl who, immediately recognizing her inferiority in building, apparently lost interest, and would not build. But she understood the problem and the competitive situation. She showed it by rather outspoken remarks and by saying whatever good of herself she could find to say.

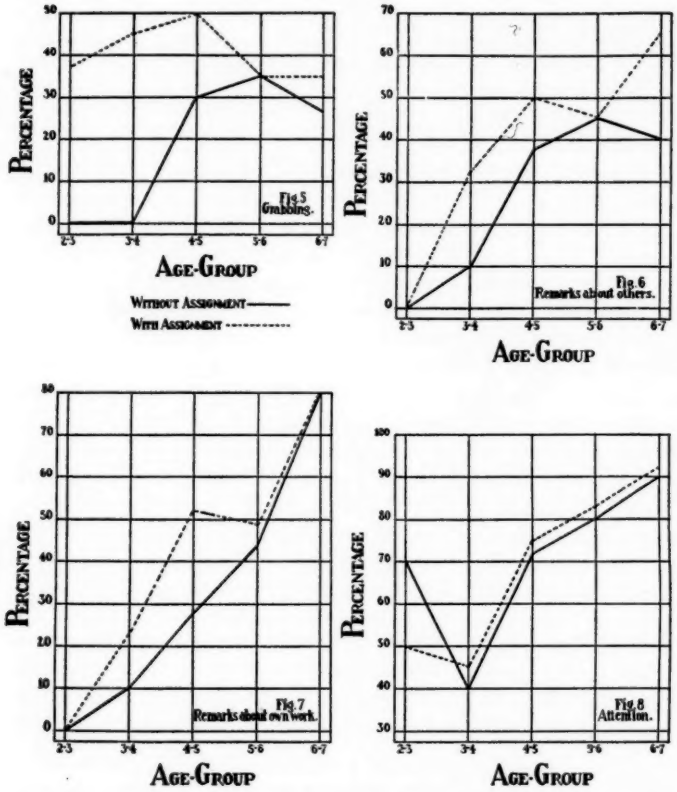
Principle 2. The second contention in the analysis of the factors causing variations in the display of competition is that competition seems to be a human inclination which is nurtured and directed by education. Hurlock has shown that children do better either after reproof or praise than after mere repetition; reproof and praise are, of course, implied in the educational method.[9] The results of the present experiments corroborate Hurlock. Instead of either praise or reproof, however, *E* injected the element of suggestion by presenting a task, an 'assignment' at the second attempt at building. The first time the two children built, they simply worked or played at random. After they had finished, they were asked whose was prettier, and then were permitted to build again. The mere asking of that question, "Whose is prettier?" is an incentive to a sensitive child. But to increase the effect, at the second building *E* said to the children, "Let us see who can build better," or "Would you like to see who can build better?" *E* saw a really appreciable increase in interest, in spontaneity, in energy, and, of course, in competition. We present in Figs. 5–6, the graphs of some of the essential criteria in establishing the presence of competition. In each case the curve representing the first building is to be compared with the curve representing the building after the assignment was given.

If the first contention, that the display of the competitive impulse is correlated positively with the command of the material, is true, then it follows that education in the use of certain material would increase the likelihood of the competitive impulse in the situation. With increased technical skill comes increased competition. There were a number of cases where each succeeding experiment showed such a marked increase in the competitive impulse that *E* permitted the children to try many times. Perhaps the reader can gather something of the tenseness, the speed, the competition in the air, the feeling that came so impressively to *E*. Note especially the kind of personal remarks made, the grabbing and hiding of stones, the increase of muscular tension, and the final out-and-out quarreling.

Girl, H., age, 4·8; boy, K., age, 4·10. No assignment given by *E*. H. began to build at once. Held up some big stones and said "I have two big stones." K. immediately said, "I have three."

H. talked while building. Said, "Here is my bed; a big bed." Handled stones carefully.

[9]Hurlock, *op. cit.*



FIGS. 5-8. SHOWING DISTRIBUTION OF THE VARIOUS CRITERIA OF COMPETITION AMONG THE DIFFERENT AGE-GROUPS

E said to K., "That's too high, it will fall." K. worked without talking. H., however, continued the conversation, "But mine isn't too high." "Later I'll make a bed." She seems to be building a fish-bath, for she said, "Here is the water and little fishes in it." To *E* she said, "Look here!"

E asked, "Are you through?" H. replied, "No, I'll be through in a minute." K. did not reply until *E* said to him, "And you?", when he simply said, "No."

When the building was finished, the children were asked which is prettier. Each considered his own the prettier.

Same children, same day.

E said to both, "I'd like to see who can build the bigger!" Both were eager to build. H. said to *E*, "Look at what I've built." K. watched H. K. grabbed a stone, and said "This belongs to me!" K. took a pretty stone, but gave another. H. said, "I'm not through yet." Sang, "Look here! look here! I'm building something fine!"

K. handled stones very carefully. Put stones in his lap when rebuilding or improving.

H. said to a stone, "You! You must stay there;" and to K., "Get away, you take too much." K., however, did not protest.

E asked "Which is bigger?" H. said quickly, "Mine is bigger;" and K. said "No, mine is bigger and wider." *E* said to H., "H., isn't K.'s bigger?" She smiled and said "Perhaps," and then changed her mind for she said, "Oh, no, mine is bigger." She pointed to a room in her construction, and said, "Here!" (K.'s construction was much larger.)

Same children, later in the same day.

E said, "I'd like to see once more who can build bigger." K. replied, "Wider;" and H., "Now I'll build wider, he will not." Both worked eagerly. K appeared to study the problem carefully. Arranged and rearranged the stones to make the building larger. Took many stones and put them in his lap. Looked up and smiled at *E*.

H. didn't talk much; once, however, when some stones fell, she said to the stones, "You! You!"

E asked K., "Are you through?" K. replied "Yes." (His stones were used up.) He took another stone from H.

E asked, "Which is bigger?" (K's building much larger.) H. said, "Mine is bigger." K. said, "No! Mine is!"

The experiment was repeated a fourth time.

H. took a stone from K., he fought with her and took some of her stones. They quarrelled. H. spoke to herself. Both built rapidly and with tense muscles. K.'s building was much larger, yet each child considered his own the bigger.

The way *E* puts the question is also of some importance. Probably the degree of the competitive impulse can be intensified by the behavior of the educator or person who has to deal with the child. Because no data were to be taken on this point, *E* made a decided effort not to inject her personality into the giving of the assignment. She always presented the questions in a cool and unimpassioned manner. But there were a few occasions when, through interest in the process, there slipped into those impartial questions a heartiness or an eagerness to which the child immediately responded. Sometimes two children who had been really apathetic during the first building showed a much more competitive attitude if *E* said with a rising inflection and interested smile, "Wouldn't you like to try again?" This was not a part of the planned experiment, but was too evident and interesting to be allowed to pass unmentioned.

It might seem that if one could know the degree of the child's mastery of the material and the degree of his understanding of the problem, and also the training he had had in the specific building situation, one could predict the amount and kind of competitive impulse he would display. But here enters the last and most subtle of the factors affecting the display of competition. The competitive impulse is not only a variable related to chronological and physiological factors, and to education in the matter in hand, but is also quite dependent on individual differences, the causes of which constitute the crux of the situation.

Here are two girls who have been put into the same situation. They are the same age; they have reached the same psychological development; they both understand the problem and build about equally well. Yet one child exhibits a marked competitive impulse, and the other very little. They have been given an assignment to see who could build larger. They are not building *together*; each is building with another child. The factors are as nearly constant as can be ob-

tained; same time of day, same month. Both families are in poor financial condition, but both children seem well and normal.

Girl, B.W., age 4·6. *E* says, "Let's see who can build bigger." B.W. looks around the room. Builds, but with great uncertainty. Sighs as she builds. Continues in this way. When asked, "Whose is larger?" she is undecided.

Compare this with the tenseness and evident eagerness to excel shown by H., just reported. B.W. is rather interested, but passive and unemotional in her response. She fulfills none of the criteria of the competition profile. H. is interested instantly and shows a clearly marked competitive impulse. She is eager, talks positively about herself, grabs stones, makes characteristic movements such as talking to the stones, makes negative remarks about the other child. She considers her own building bigger, even though that of the competing child is twice as large.

This variation in the competitive impulse can be seen at a much earlier age. Both of the children in the next comparision are 3½ yr. old, and neither one has reached a fully developed competitive stage, but clearly one tends very much more in that direction than the other.

Boy, H., age 3·6. *E* says, "I would like to see who can build better. Can you build a train?" H., "Oh! Yes!" Sings, "Train! Train! Train!" Makes a noise like steam. Stands up to build. Movements become sharper, more eager. Says, "I am building a locomotive." Fumbles somewhat with the stones, but goes on building. Other child observing this fumbling says, "Can't you even build?" H., very curtly, "Yes." Stops building. Comes to side of *E*. Stands by her side. *E* asks, "What is that?" Answer, "A train." Adds another stick then says, "So! now I have a window; I must build a little more." Suddenly to *E*, "Isn't mine pretty?" Sings: "A train, an express train!" *E*: "Aren't you finished yet?" Answer: "No, I will stand and build." *E* asks both children, "Which is prettier?" Other child answers, "Mine." H. says quickly, "No, mine."

In the construction of this 3½-yr.-old boy there was something to be seen, but the result was not in proportion to the certainty and assurance of this little fellow that he was making "an express train."

Girl, R., age 3·6. *E* says, "Who can build the nicest train?" R. looks about. Seems somewhat uncertain. Glances at B. (her partner) to see what he is making. Goes on quietly trying to build but not with great success. Other child invites her to build on the floor. She goes on building on the table. When finished, both children are asked to stand by *E* and to look carefully at each other's constructions. Question is put to R. first, "Which is prettier?" She smiles shyly at *E* and points to her own.

There is a decided difference in tone between the attitudes of these two children. H. is perhaps not much more advanced than R. in constructive ability, but his whole attitude is very unlike hers. He is determined and aggressive. He builds and rebuilds. He looks at no one, gives his construction a name and builds straight toward an end. He makes positive remarks about his own work. The girl, R., is uncertain; she glances at the other child to watch and copy. She tries nothing new, such as the floor as a place whereon to build, even though it is suggested to her. The boy is experiencing the first phases of the competitive feeling. He wants to make his work as fine as he can. The girl is not quite oriented in the idea of excelling. The cases illustrating this point are numerous. Here is a girl of 6·2 yr., who is capable and interested and yet exhibits no competitive signs at all, and another girl of 6·9 yr. who exhibits competition to a most marked degree, etc.

Bühler includes a discussion of types of children in her study on "Despotism and Rivalry." Even in the case of babies she differentiates between the behavior of those she terms the despots and of those who are passive and receptive. She mentions a third type who bend everything before them through their activity. A child of this type overcomes children both older and younger through the very intensity of his far-reaching activity.

It is somewhat difficult in so complex a situation to say exactly what causes these variations in temperament. It seems, to *E* at least, that one must at the outset realize that the conditions prevailing at the time play a tremendous rôle in what may appear as 'type' conduct. Such a case is C., who apparently had no interest in building, and certainly evidenced no competitive spirit. This child's apathy is certainly better understood if the fact be reported that 10 min. later she came and threw herself into *E*'s arms sobbing out her lonesomeness. Her apathy was not apathy. It was grief. The disinterested child may not be disinterested; perhaps he is hungry or tired, or misses his mother, or is new to the Institution. Maybe he wants to go out in the garden. There are a thousand possibilities.

Yet despite all that, after observing daily for months the behavior of these children as they build together (and the frequency of the observation lessens the importance of chance factors) the observer can report a difference in the individual temperaments of the children. Some children have a strong desire to excel, a desire that manifests itself by stealing, hiding, making nasty remarks, getting "hot and bothered" when competing. Other children have a desire to excel but manifest it differently. These children simply give their attention to the blocks. They build and rebuild. They make some remarks, but neither so many nor so disagreeable as the first group. They take many stones, but they do not rush so fast in their grabbing, and they often ask before they take. A third type is not competitive at all. Sometimes we can offer no explanation except in terms of the child's general disposition.

These types may be the result of inborn differences, or they may be the result of social conditioning. The differences to be observed in children of 2-yr. of age (or, of course, older) are, in our opinion, the results of both. One has only to watch babies in their cribs, and certainly children at play, to recognize variation in temperament. But with some degree of certainty we can state that the tendency to compete can be developed in a child; that it did grow in some children as we observed them perform their building a repeated number of times.

Summary

By competition we mean that effort manifested by a child when he is influenced by the 'desire to excel.'

The criteria for the presence of the competitive impulse are:

(A) An understanding of the problem, determined by (1) the way the material was attacked; (2) the use made of the material; and (3) the answer given to the *E*'s question.

(B) The interest shown in the problem, judged by:

(1) Attention during building, which in turn was measured by (a) lessened disturbance by outside influence; and (b) consistency in building.

(2) Physical signs of interest, *e.g.* (a) tensity of muscles; (b) increased speed; and (c) unusual or characteristic gestures.

(3) Remarks showing interest. (a) Positive or negative remarks about other child; and (b) positive or negative remarks about own work.

(4) Technique in building. (a) Building and rebuilding; (b) grouping of stones; (c) evident desire to go on constructing even after other child is finished.

(C) A competitive attitude toward the other child, viz. (1) watching or copying; (2) grabbing or stealing stones from other child; (3) offer of help or of stones; (4) disagreeable or critical remarks; (5) gestures of quarreling.

The results of the study reveal:

(A) Competition in building with blocks is not found in children of all ages, nor in all children of any one age.

(B) There appear to be 4 fundamental factors affecting the display of competition in building with blocks: (1) the degree of the child's understanding of the idea of excelling; (2) the degree of the child's ability to dominate the material; (3) the educational factors in the situation; and (4) the individual temperament as a competitive factor.

(C) The growth of the competitive impulse in the use of building stones seems to follow a well defined and orderly course, as follows: (1) no competition; the child discovers himself in relation to the material and is interested in functioning with that material (age-group 2-3 yr.); (2) some competition; a little better understanding of the idea of excelling; child "discovers" the other child, and although his constructive ability increases he is more interested in the social relationship than in the competitive situation (age-group 3-4 yr.); (3) competition; the child discovers the work and manifests his desire to excel (age-group 4-6 yr.); and (4) competition: with increased critical judgment (age-group 6-7 yr.).

(D) In the use of building stones no competition was seen at all in children of two years of age. At least 90% of 6-yr.-olds have it well developed. It can be said to make its appearance usually at about the age of 4 yr.

THE RELATION OF MAZE ADAPTABILITY, MAZE LEARNING, AND GENERAL INTELLIGENCE

By Richard S. Schultz, University of Pennsylvania

The general purpose of this investigation was to study a number of factors related to maze learning.[1] In particular, it was concerned with two problems: (1) the relation of general intelligence to ability in solving a maze problem and in maze learning; and (2) the relation of the ability to *solve* a maze problem to the ability to *learn* a maze problem.

This investigation differs from previous studies in its emphasis on the relation of adaptive capacity to maze learning. It attempts to obtain a measure of motor adaptive capacity as well as of motor learning in the same task.

Following earlier studies, a general intelligence test is used as one expression of adaptive capacity. But, in addition, another expression of adaptability is obtained, in a series of five maze problems, through the use of the Miles Duplicate Maze.

Historical Review

In two independent studies, the coefficients of correlation between maze learning ability and general intelligence range from -0.30 to -0.45.[2] Another study has approached the problem of general adaptive capacity in maze learning by a comparative analysis of the performance of children and adults. In this investigation the conclusion is reached that extreme degrees of intelligence may be handicaps in maze learning.[3]

A study of the relation between intelligence and the ability to solve maze problems is reported by Cox.[4] The Miles Duplicate Maze is presented in a series of five problems of varying difficulty. This situation is not one essentially involving learning; but rather an attempt to measure the intelligence factors

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[1]From the Psychological Laboratory of the University of Pennsylvania. The writer wishes to acknowledge his indebtedness to Professor Edwin B. Twitmyer, Dr. Morris S. Viteles, and Professor Samuel W. Fernberger, and to Dean Charles R. Turner, of the School of Dentistry.

[2]W. S. Hunter, Correlation studies with the maze in rats and humans, *Comp. Psychol. Monog.*, 1, 1922 (no. 1), 37-56; C. J. Warden, Relative economy of various modes of attack in the mastery of a stylus maze, *J. Exper. Psychol.*, 7, 1924, 270-272.

[3]M. C. Gould and F. A. C. Perrin, A comparison of factors involved in the maze learning of human adults and children, *J. Exper. Psychol.*, 1, 1916, 122-154.

[4]C. M. Cox, The intelligence factor in the solution of space problems with the two-story maze, this Journal, 40, 1928, 542-561.

involved in the solution of a relatively new maze problem. The correlations obtained in this earlier study between performance time and the Thorndike College Entrance Test are given in Table I in the report of the present investigation. A qualitative analysis has also been made of intelligence factors involved in these maze problems. Cox suggests that the Miles Duplicate Maze may serve as a valuable performance test of intelligence, if properly standardized.

A number of investigations on the maze indicate the presence of such factors as distractability, temperament, general emotional tone, fatigue, methods of attack, interpretation of problem, which complicate the learning process.[5] Boring, in a study of maze learning with dementia precox subjects, found it advisable to use a paper maze as a preliminary device in order to create a proper attitude and to familiarize the subject with the general nature of the task.[6] Warden, in work with the white rat on the problem box,[7] compared a preliminary adjustment period of 7 initial exploratory trials or standard feeding periods for one group with 7 regular trials of a control group in learning. The results indicate that a period of random activity at the beginning in learning serves to eliminate emotional disturbance, hastens formation of habit pattern, and reduces the total learning time 60%. Methods of extraneous control in learning acts of motor skill are reviewed by Carr.[8] The paper is especially concerned with an evaluation of various forms of 'tuition' (guidance, directions, information, attitude, etc.) and incentive as they are related to the learning process in the maze situation. Perrin also considers various techniques and methods of presentation in learning the stylus maze.[9]

Method

Apparatus. The apparatus, the Miles Two-Story Duplicate Maze, consists of two identical maze patterns, one mounted directly above the other on a wooden box with two open sides. A black cloth fastened to the top platform falls over the open side parallel with *S*'s body, and covers the arm and hand on the bottom maze. The opposite open side gives *E* a full view of the bottom maze.

A small key-piece, an exact replica of the center of the maze, is attached to the top platform and serves as an index to the position of the bottom maze. The lower maze is mounted on a large rotating disk and can be turned to any desired position in reference to the stationary upper maze.

The task involves the movement of a stylus through a visible top pattern, while at the same time another stylus (with a rubber disk to eliminate contact with the maze) is moved through the screened bottom pattern. The path traced through the lower maze is recorded on a sheet of paper placed under the pattern.[10]

[5]Gould and Perrin, *op. cit.*, 122-154; also Perrin, An experimental and introspective study of the human learning process in the maze, *Psychol. Monog.*, 16, 1914 (no. 70), 36 ff.; Hunter, *op. cit.*, 50-52.

[6]E. G. Boring, Learning in dementia precox, *Psychol. Monog.*, 15, 1913 (no. 63), 51-55.

[7]C. J. Warden, The value of the preliminary period of feeding in the problem box, *J. Comp. Psychol.*, 5, 1925, 365-372.

[8]Harvey Carr, Teaching and learning, *Ped. Sem.*, 37, 1930, 189-218.

[9]Perrin, *op. cit.*, 36 ff.

[10]The top maze was mounted on glass, thus eliminating a scratched surface which would obviously become a disturbing variable. The large disk for the bottom maze had to be set with extra screws around the edge and a special fastener in order to make it more rigid and secure. Four extra studs were also attached to the disk in order to hold the bottom maze more rigidly.

The arrangement of the two mazes and the use of two hands in operation has the advantage of presenting a problem involving the visual, tactile and kinaesthetic senses. This has been classed by Miles as a motor task with maximum indirect visual guidance.[11] The use of cues from one hand to the other, in addition to visual, tactile, and kinaesthetic senses, possibly make the guidance element more complex.[12]

Procedure. The maze task is divided into two parts. The first part is referred to as an 'adaptability' task. The *Ss* are required to trace through 5 successive positions of the maze as quickly as possible. These problems of varying difficulty are obtained by rotating the bottom maze to the following angles in a counter-clockwise direction—0°, 180°, 90°, 180° reverse, and 90° reverse. The last two problems are mirror reversals of the 180° and 90° positions. In presenting the 5 positions of the maze, an attempt is made to test the various abilities and traits that may be evoked in adaptation to the Miles Duplicate Maze situation. In general, the instructions and procedure for the adaptability task follow that of the previous investigators.[12]

After the adaptability task, there is an approximate 5-min. rest interval. In the second part of this experiment one of the above problems (180° reverse position) is given as a 'learning' task. Standard instructions and procedure for the learning task were developed by the present investigator.

The test score in 'adaptability' is based on the time taken, in one presentation of each problem, to trace from the center through to the last exit of the maze. An adaptability problem not solved in 900 sec. was recorded as a failure. In 'learning' the number of trials and the time per trial were recorded. The learning task is finished when two successive trials are made without any retraces and entrances into blind pathways. A detailed record was kept on a standard mimeographed form of the quantitative and qualitative aspects of the performance.

General intelligence. The Psychological Examination (for High School Graduates and College Freshmen), American Council on Education, was used. It was administered, before any maze tests were given, as a group examination.

Subjects. The Main Group of *Ss* for this investigation includes 89 men, students in the Dental School, University of Pennsylvania. All were in the second year class. They had also completed 2 years of a college pre-dental course. A number of them are college graduates and several have pursued graduate and specialized courses. They averaged about 22½ years old and ranged from 20 to 28 years in age.

A second group of *Ss*, 11 in number and hereafter referred to as the Experimental Group, was used in one part of this experiment. This group has a median age of 20 with a range of 17 to 27 years. It consists of 9 men in the undergraduate schools, an instructor, and a college graduate.

Results

(*1*) *Maze adaptability, general intelligence, and chronological age.* An examination of Table I, for the Main Group, indicates that the two

[11] W. R. Miles, The two-story duplicate maze, *J. Exper. Psychol.*, 10, 1927, 365-377.

[12] C. M. Cox, *op. cit.*, this Journal, 543-545; Comparative behavior in solving a series of maze problems of varying difficulty, *J. Exper. Psychol.*, 11, 1928 203-206; Miles, *op. cit.*, 371-374.

separate total maze adaptability time-scores and the gross time-scores for each problem correlate insignificantly with intelligence test scores.[13]

TABLE I

CORRELATION OF THE INTELLIGENCE TEST WITH THE TIME-SCORES IN THE VARIOUS MAZE PROBLEMS, AND COMPARISON OF THE RESULTS FOR THE MAIN GROUP OF *Ss* WITH COX'S RESULTS

	Problem				
	0°	180°	90°	180°R	90°R
Main Group	−.18±.07	−.18±.08	−.04±.08	−.19±.07	+.34±.07
Cox's Results	−.26±.06	−.59±.05	−.40±.07	−.11±.09	−.09±.09

	Total Score		
	Gross	Weighted	Av.
Main Group	+.04±.09	+.03±.09	
Cox's Results			−.39±.09

TABLE II

COMPARISON OF TOTAL GROSS TIME-SCORE IN THE FIRST FOUR PROBLEMS WITH THE GROSS TIME-SCORE IN THE 90° REVERSE PROBLEM FOR GROUPS CLASSIFIED IN THIRDS BASED ON INTELLIGENCE TEST SCORE

(70 *Ss*, the odd *S* was grouped in the middle third)

	First four problems			90° R problem		
	Upper	Middle	Lower	Upper	Middle	Lower
Average	726.5	1077.9	949.0	410.8	364.6	242.4
Median	585.0	1047.0	882.0	333.0	401.0	197.0
75% score	426.0	579.0	466.0	185.0	191.0	121.0
25% score	1056.0	1344.0	1206.0	608.0	504.0	322.0
Quartile deviation	315.0	382.5	370.0	211.5	156.5	101.5
Range	342–1290	311–2200	242–2670	97–862	64–806	75–796

The correlation of intelligence test score with performance time on the 90° reverse problem deserves closer examination. Table II shows that the upper third of intelligence has spent less average time than the middle and lower thirds on the first four adaptability problems. But in the last problem (90° reverse position) in which only

[13]A total adaptability score was obtained in two ways. (1) The time scores for each individual on the 5 problems were simply added together. (2) Another total score was obtained by finding the ratio of the S.D. for each problem to the S.D. of the 0° problem. The raw score of each problem was then multiplied by its respective ratio. All the problems are, therefore, given a relatively equal value in the total score. These weighted scores added together yield the total weighted score. The validity of this method was ascertained by correlating the total weighted scores with their respective gross scores for the 55 *Ss* successful in all adaptability problems. This gave a correlation of +0.94, P.E. 0.01. The total scores are referred to respectively as the gross score and the weighted score.

70 *Ss* were successful, the average time score is higher for the upper third of intelligence and lower for the middle and lower thirds. The more time used in tracing the maze in the four positions preceding the 90° reverse problem may possibly lead to greater familiarity with the maze pattern in general; and consequently influences the performance time on the last adaptability problem.[14]

Intelligence test scores may be compared in another way with maze adaptability performance. The number of failures in each maze adaptability problem is tabulated, in Table III, for the intelligence test quartiles. One fact stands out clearly in this comparison. The highest quartile of intelligence has decidedly less failures in all maze adaptability problems.

TABLE III

NUMBER OF FAILURES IN MAZE ADAPTABILITY COMPARED WITH QUARTILES OF INTELLIGENCE TEST

(89 *Ss*, the odd *S* was grouped in the Third Quartile)

	Quartile Rank in Intelligence Test				
Problem	Lowest	Second	Third	Highest	Total
0°	0	0	0	0	0
180°	2	2	1	0	5
90°	7	8	10	1	26
180° R	1	2	1	0	4
90° R	6	5	6	2	19
Total	16	17	18	3	54

The results on the relation of general intelligence to maze adaptability do not, in general, agree with Cox's.[15] The following facts may be of significance in this connection. The Main Group of *Ss* was selected as to intelligence.[16] In maze adaptability, this group is superior in performance to 2 other groups (see Table IV).[17]

[14]Perrin, *op. cit.*, 36 ff. After learning a maze pattern, a change in the position of the maze or a placement of the stylus in various parts of the maze gave little difficulty. The *Ss* were able to adjust to the changes readily, showed signs of familiarity with the paths and the general pattern, and the speed of tracing through the pattern closely approximated, in a short time, the speed for the originally learned maze position.

[15]Cox, *op. cit.*, this JOURNAL, 542-546; also *op. cit.*, *J. Exper. Psychol.*, 206-208.

[16]L. L. Thurstone and T. G. Thurstone, The 1929 psychological examination, *Educ. Record*, 11, 1930, 107-108. When the general intelligence test scores are compared with the larger mixed group of 34,507 cases from 131 colleges, cited in the above reference, the average and median scores of our *Ss* rank above the 75th percentile.

[17]Influence of training appears most important as a possible explanation for any significance that may be attached to the above facts. The *Ss* in the Main Group have had special training, to a very great extent, in tasks involving a variety of motor skills. They have also been trained to explore and to make

TABLE IV

SIGNIFICANT MEASURES BASED ON SUCCESSFUL CASES: COMPARISON OF THE RESULTS OF THE MAIN GROUP, THE EXPERIMENTAL GROUP, AND COX'S GROUP

	Main Group				
Problem	No.	Av.	Median	S.D.	Range
0°	89	67.0	55.8	41.0	20–217
180°	84	217.1	150.0	180.6	33–876
90°	63	420.7	336.0	264.4	56–895
180° R	85	166.4	120.0	136.8	36–698
90° R	70	341.2	300.0	212.4	64–862
Gross total score	55	1054.5	1100.0	416.7	317–1987

	Experimental Group				Cox's Group*	
Problem	No.	Av.	Median	Range	Av.	S.D.
0°	11	51.1	54.0	21–105	96	92
180°	10	337.2	274.0	95–584	361	311
90°	7	596.0	545.0	290–871	463	257
180° R	10	284.2	179.0	42–829	217	179
90° R	7	559.1	561.0	149–850	432	298
Gross total score	6	1720.3	1377.0	1202–2227	—	—

*Consists of 67 college students, 39 men and 28 women.

There is, as is shown in Table V, a low correlation between chronological age and maze adaptability. It reveals a tendency for a lower (better) time score to be associated with a higher chronological age, and vice versa. A consideration of Table I combined with the results in Table V suggests that the degree of success in maze adaptability is favored by a low chronological age and that intelligence is negligible.

A further analysis of the relation of intelligence and chronological age to maze adaptability is found in the comparison of successful and

TABLE V

CORRELATION OF CHRONOLOGICAL AGE WITH TOTAL MAZE ADAPTABILITY SCORE FOR CASES SUCCESSFUL IN ALL FIVE PROBLEMS

	r	P.E.$_r$
Total gross time score.....	−.25	.08
Total weighted time score.....	−.25	.09

delicately controlled movements in unseen or limited visual fields with the use of a mirror for work in teeth cavities. However, the available data do not warrant an assumption that any specific transfer or even transfer of general attitudes has directly influenced the results of this group. Correlations of the total weighted adaptability time-score with 8 criteria of technical abiilty in Dental courses range from −0.25, P.E. 0.09 to −0.37, P.E. 0.08.

failure groups (failure in one or more problems). Success as contrasted with failure in maze adaptability appears to be favored by a low chronological age and a high intelligence test score (see Table VI).

TABLE VI

COMPARISON OF THE INTELLIGENCE TEST SCORE AND CHRONOLOGICAL AGE OF THE 55 *Ss* WHO SUCCEEDED IN THE ADAPTABILITY PROBLEMS WITH THE 34 *Ss* WHO FAILED

	General intelligence (in points scored)		Chronological age (in months)	
	Failed	Succeeded	Failed	Succeeded
Average	181.8	193.3	277.7	269.4
Median	182.9	193.3	271.3	267.3
S.D.	44.8	57.2	19.0	17.7
Range	75–268	57–299	244–338	239–317
D/σ diff.		+1.06		+2.05

(*2*) *Maze learning, maze adaptability, general intelligence, and chronological age.* The data that follow present an analysis of the relation between the ability to *solve* a maze problem and the ability to *learn* a maze problem. In the literature, there appears no report of such a comparison of maze learning and maze adaptability with the Miles Two-Story Duplicate Maze or with any similar apparatus.

The 180° reverse position was selected as a learning task following preliminary experimentation with the other four positions of the maze. The problem ranks second in difficulty based on average time score (Table IV). It correlates highest with all of the adaptability problems, with the exception of the 90° reverse position, as shown in Table VII.[18]

TABLE VII

INTERCORRELATION OF TIME-SCORES ON THE FIVE PROBLEMS FOR THE MAIN GROUP

Problem	0°	180°	90°	180° R
180°	+.31 ±.07			
90°	+.18 ±.08	+.22 ±.08		
180° R	+.45 ±.06	+.43 ±.06	+.34 ±.08	
90° R	+.10 ±.08	−.02 ±.08	+.31 ±.08	+.19 ±.08

[18]The general trend and magnitude of these intercorrelations agree very closely with previous findings by Cox, *op. cit.*, this JOURNAL, 545-547. W. T. Heron (Ability *vs.* chance in learning the stylus maze, *Comp. Psychol. Monog.*, 2, 1924 (no. 8), 14-17) reports a similar magnitude of intercorrelations for time scores in five slightly different maze problems that were completely learned. C. L. Hull (*Aptitude Testing*, 1928, 215) says, "While experimental results have failed to show any convincing evidence for distinct levels of abilities, it has revealed what may turn out to be a significant tendency or law, this is that highly complex intellectual activities correlate highly with each other, the less complex correlate to an intermediate degree, and the relatively simple motor activities correlate with each other only slightly."

The low correlations in Table VIII indicate a positive relationship between performance in maze adaptability and maze learning. Group comparisons appear more significant.

TABLE VIII

CORRELATION OF TOTAL TIME-SCORE IN ADAPTABILITY WITH TOTAL TIME-SCORE IN LEARNING

(37 selected *Ss*)

	r	$P.E._r$
Total gross time-score.....	+.11 (+.24)	.11
Total weighted time-score.....	+.19 (+.25)	.11

The total weighted adaptability scores for the select successful group[19] are arranged in rank order and then divided into approximately equal thirds. Comparisons of the maze-learning data are then made possible by distributing the learning scores under these three maze-adaptability sub-groups.[20]

TABLE IX

FIRST PERFECT TRIAL: COMPARISON OF TIME AND NUMBER OF TRIAL FOR THE SELECT SUCCESSFUL GROUP, DIVIDED INTO THIRDS ON THE BASIS OF TOTAL WEIGHTED ADAPTABILITY SCORE

(37 *Ss*, odd *S* grouped in middle third)

	Time			Trial		
	Upper	Middle	Lower	Upper	Middle	Lower
Average	41.8	47.9 (49)	59.9 (58)	11.5	11.5 (10)	16.5 (15)
Median	37.0	47.0 (38)	34.0	6.0	7.0	14.0
75% score	15.0	21.0	24.0	2.0	5.0	4.0
25% score	51.0	49.0	67.0	17.0	11.0	23.0
Quartile deviation	18.0	14.0	21.5	7.5	3.0	9.5
Range	14–100	9–149	18–162	1–34	1–26 (24)	1–42

[19]This group of 37 cases has been especially isolated because all the *Ss* were successful in both adaptability (all 5 problems) and learning. Whatever relations are revealed between adaptability and learning should appear more clearly with this group, as the conditions of relative and complete achievement were uniform for it.

[20]In examining the various data on learning, the nature of the distribution of the scores and the effect of removing 2 extreme cases may be considered. The total learning times scores are massed toward the lower end of the distribution. Two extreme cases occur beyond the upper range of 3 S.D. These 2 cases fall in the middle and lower sub-groups based on total weighted adaptability score. When these 2 scores are eliminated the differentiation among the three sub-groups in Tables IX, X, XII, and XIII, becomes less distinct. But the indicated general trend in the various comparisons persists. The manner in which the coefficients of correlation are affected by this change is shown in Tables VIII and XIV. Figures in parenthesis indicate the effect of removing the 2 extreme cases.

The various factors in the learning data, in Table IX and X, are consistently differentiated by the above classification. The largest differences appear between the upper and lower sub-groups.

TABLE X

COMPARISON OF TOTAL TIME, TOTAL NUMBER OF TRIALS, AND AVERAGE TIME PER TRIAL FOR THE SELECT SUCCESSFUL GROUP DIVIDED INTO THIRDS ON THE BASIS OF TOTAL WEIGHTED ADAPTABILITY SCORE

(37 selected *Ss*, the odd *S* was grouped in the middle third)

	Total time			Total trials			Av. time per trial		
	Upper	Middle	Lower	Upper	Middle	Lower	Upper	Middle	Lower
Average	831.6	1266.7 (970)	1816.6 (1479)	19.2	23.4	32.5 (26)	50.2	66.9 (62)	79.8 (81)
Median	733.0	881.0 (822)	1425.0	18.0	12.0	24.0	49.0	64.0 (56)	61.0
75% score	623.0	492.0	722.0	11.0	7.0	8.0	31.0	47.0	40.0
25% score	1138.0	1181.0	1875.0	28.0	25.0	43.0	57.0	70.0	95.0
Quartile deviation	257.5	344.5	576.5	8.5	9.0	17.5	13.0	11.5	27.5
Range	251–1698	251–4831 (2481)	370–5526 (3208)	3–35	2–82	4–73 (60)	31–84	16–131 (119)	39–180

In Table XI all of the cases successful in learning are divided into two groups based on performance in adaptability. The group with failures, in one or more adaptability problems, consumes not only longer time, necessarily, in adaptability, but also in learning. This agrees with Table XV, which shows that the failure group in learning was the poorer group in adaptability. As in the preceding data, both Tables XI and XV reveal a positive relationship between performance in maze adaptability and in maze learning.

TABLE XI

COMPARISON OF TIME-SCORES IN LEARNING AND ADAPTABILITY

	Adaptability time		Learning time	
	Success in both	Failure in adaptability	Success in both	Failure in adaptability
	(37 *Ss*)	(16 *Ss*)	(37 *Ss*)	(16 *Ss*)
Average	1060.8	2099.1	1301.5	1457.8
Median	1125.0	2182.0	969.2	1168.0
75% score	725.0	1637.0	575.0	763.0
25% score	1300.0	2281.0	1400.0	1931.0
Quartile deviation	287.5	322.0	412.5	583.0
Range	372–1987	1425–2803	237–5526	365–2895

The errors should give information,[21] not revealed in trial and time scores, concerning the relation between maze adaptability and maze learning. In Table XII, the lower third when compared with the upper and middle thirds makes more errors in learning and eliminates errors more slowly as shown by the percentage of cases that complete learning by the 35th trial.[22]

TABLE XII

COMPARISON OF THE SELECT SUCCESSFUL GROUP, DIVIDED INTO THIRDS ON THE BASIS OF TOTAL WEIGHTED ADAPTABILITY SCORE

(37 *S*s, odd *S* was grouped in middle group)

	Thirds		
	Upper	Middle	Lower
Total no. errors	635	676 (571)	978 (639)
% successful by the 35th trial	100	80 (83.3)	58.3 (63.6)

A more direct presentation of the number of errors eliminated and the rate of error elimination is revealed in Table XIII which shows (1) the total number of errors for every 5 trials, and (2) the ratio, in percentage, of the total number of errors in the first 5 trials to the total number of errors in every succeeding group of 5 trials.

TABLE XIII

TOTAL NUMBER OF ERRORS FOR EVERY 5 TRIALS AND RATIO IN PERCENTAGE OF ERRORS IN FIRST 5 TO ERRORS IN SUCCEEDING GROUPS OF 5 TRIALS, FOR THE SELECT SUCCESSFUL GROUP DIVIDED INTO THIRDS ON THE BASIS OF TOTAL WEIGHTED ADAPTABILITY SCORE

(37 *S*s, the odd *S* was grouped in the middle third)

	Upper third		Middle third		Lower third		Entire group	
Trials	Total errors	% ratio	Total errors	% ratio	Total errors	% ratio	Total errors	% ratio
1-5	201	100.0	244 (224)	100.0	238 (170)	100.0	683	100.0
6-10	141	70.2	122 (110)	50.0	185 (135)	77.7	448	65.4
11-15	106	52.8	87 (68)	35.7	152 (92)	63.8	345	50.4
16-20	98	48.8	78 (63)	32.0	125 (64)	52.5	301	44.0
21-25	63	31.4	56 (40)	23.0	117 (71)	49.1	236	34.5
26-30	22	11.0	51 (37)	20.9	87 (59)	36.5	160	23.4
31-35	4	2.0	38 (29)	15.6	74 (48)	31.1	116	16.9

[21]The study of errors has been confined to entrance into blind pathways with the exception of one retrace error. This is an elbow turn leading from 6a true path before entering exit path 6. A diagram of this maze pattern is presented by Miles, *op. cit.*, 371.

[22]Only 35 trials of the learning curve have been included, the range of trials is 2-82. The number of trials, however, are representative of the complete learning curve, since 78% of the group (29 out of 37 *S*s) learned before or at the 35th trial.

The error curve for all sub-groups shows a sudden drop in the first 10 trials. This agrees with the fact that 34% of all the perfect tracings were made in the first 10 trials. The various comparisons indicate that the lower third, slowest in adaptability, makes most errors in learning; has most errors to eliminate; and eliminates errors at a slower rate. The middle third is superior to the upper third up to the 25th trial. At this point the error curve drops suddenly for the upper third.[23]

The coefficients of correlation in Table XIV are low and statistically unreliable. They signify only a possible trend of agreement between lower total learning time (better performance), higher intelligence test score, and higher chronological age.[24] A similar tendency appeared with the data on maze adaptability. But intelligence appears slightly more significant in maze learning than in maze adaptability.

TABLE XIV

CORRELATION OF INTELLIGENCE TEST SCORE AND CHRONOLOGICAL AGE WITH TOTAL LEARNING TIME-SCORE

(37 selected *Ss*)

	r	$P.E._r$
Intelligence test score	−.25 (−.35)	.10
Chronological	−.18 (−.05)	.11

In Table XV, the successful and failure groups in learning are presented for comparison.[25] The results in this table indicate that

[23]The upper third actually completes learning at the 33rd trial; errors at the 35th trial should be regarded as zero.

[24]E. L. Thorndike (*Adult Learning*, 1928, 106, 155) says, "The general tendency from all our experiments is for an inferiority of about 15% as a result of 20 years from 22 on. Learning representing an approximation of sheer modifiability unaided by past learning shows considerably more inferiority than this. Actual learning of such things as adults commonly have to learn shows considerably less." Experience, interest, caution, care, and organization are important factors possibly favoring older individuals. R. W. Husband (Certain age effects on maze performance, *Ped. Sem.*, 37, 1930, 325-328) found that *Ss* 30 years of age were somewhat inferior in maze learning to those around 20, especially in a comparison of time scores. Gould and Perrin (*op. cit.*, 122-154) discovered that adults whose average chronological age was 21 years when compared with children whose average age was 11 yr. 6 mo., in maze learning made better records in trials, time, errors, and distance traversed. B. H. Candee (The relationship of intelligence and age to efficiency in maze learning, cited in review of literature by S. Y. Liu, The relation of age to learning ability of the white rat, *J. Comp. Psychol.*, 8, 1928, 75) says, "The high school students made better records than did the children from the grades, while the college students were slightly superior to the high school students."

[25]The number of cases varied in parts of the experiment. In learning, 24 *Ss* were used in preliminary experimentation, and consequently are not included in these results.

the failure group in learning spends more time in adaptability; the learning period is decidedly longer in terms of trial and total time; intelligence test scores are lower; and age does not appear significant.

TABLE XV

COMPARISON OF THE RESULTS OF THE GROUPS THAT SUCCEEDED AND FAILED IN LEARNING THE MAZE

	Succeeded (53 *Ss*)			Failed (12 *Ss*)		
	Av.	Median	Range	Av.	Median	Range
Learning time in sec.	1365.0	1042.0	237–5526	2281.1	2123.0	1098–4077
No. trials	26.4	22.6	2–82	56.0	60.0	4–100*
Adaptability time in sec.	1377.7	1300.0	372–2803	1702.9	1696.0	424–3466
Intelligence test score	193.8	191.7	73–299	170.3	161.0	75–247
Chronological age in mos.	270.3	268.1	239–317	268.9	268.0	251–299

*The required minimum for all *Ss* was 30 trials; 2 *Ss* made 4 failure trials (900 sec.) in succession.

(*3*) *Case study of failures in learning.* This section includes a brief description and analysis of the cases that failed to achieve the learning criterion within a total test period ranging from 2–3 hr. The value of this presentation is manifold. It will present in relief the general nature of the difficulties in this motor problem, the variety of methods of procedure, and the characteristics of the unsuccessful learner. It will also serve to reëmphasize the fact that this motor problem is not a simple trial and error task; but that it evokes a complex response which is indicative of numerous characteristics, traits, and abilities of the individual.

In reading the descriptive and observational data, it is well to recall the quantitative differentiations already made between this group and the successful group in age, intelligence, adaptability, and learning (cf. Table XV).

The 12 *Ss* who failed may be divided into two groups. One, consisting of 8 *Ss*, particularly manifests temperamental, general behavior, and personality characteristics that may account more or less for their failure in learning. A brief summary for each individual in this group follows.

S 1 appeared very tense during performance. There were frequent complaints, "don't see why it won't work," "don't see how one can concentrate," "just sheer luck if I get out of here, I'm using no plan." He seems to lack persistence and planfulness. He was very disappointed in not being able to complete learning satisfactorily.

S 2 criticized and commented on his performance throughout the experiment, "this is hard," "should have watched myself," "upper maze confuses me trying

to keep my mind on lower." He knew that slight entrances were being made into vertical blinds toward body—"don't want to increase time," "disgusted with myself for making foolish errors." He claimed he is too sure of himself.

S 3 appeared very tense and hyperactive. He is easily excited; is the only child in the family. He states that he has a verbal memory defect especially in foreign languages, and difficulty of concentration. He states that he likes to talk about himself freely, but that he is shy and can't make friends easily. This and other 'factors' have been treated by a psychiatrist. During the performance he used profanity in the most abusive manner, appeared very resistive, at times almost to the point of violence; although he wanted to continue the test and give his best performance.

S 4 appeared very distractable, "can't keep my mind on this thing (maze)." He has a lethargic appearance, easily discouraged, wanted to stop work on the maze several times, complained of fatigue in hand frequently.

S 5 stated that he gets nervous in examinations. He appeared disturbed throughout the experiment, and frequently displayed restlessness by shifting feet, shifting body, twisting and turning head. His hands are very large, heavy, clammy, and have a slight tremor. He complained of task, "awful complicated," and had difficulty in coördinating movements at the beginning of the maze pattern.

S 6 stated that he is restless all the time, very fidgety, "can't sit still or read even a good book for any length of time." When an error was made, especially during learning, he would give a general bodily reaction and would ask to be returned to the beginning of the maze.

S 7 is ambidextrous. He appeared very self-conscious and lacks confidence, "I must be a pretty bad specimen." He seemed to lack persistence and ability to initiate, "if I didn't get out of the other one (90° pattern) how am I going to get out of this one (90° reverse pattern)?" "I wonder whether I'm doing what's permissible, orienting myself." (Looked away from top of maze, trial and error on lower maze). "Damned thing doesn't work," (but did not make the next move which would have been correct); seems to have difficulty in attention—"Gee, my mind seems to be off the damned thing." After the experiment he gave several instances of distractability and day dreaming in his everyday activities.

S 8 is also ambidextrous. The natural inclination was toward lefthandedness, and the right hand has been trained. Frequently he must make a conscious decision as to which hand to use in a particular task. He has a special aversion toward changing a daily habit, "develops nausea and gets nervous." Throughout the experiment he appeared nervous, would frequently tense the body, nod the head abruptly, and display facial tics and grimaces.

The remaining 4 *S*s do not give such an overt display of behavior. Two of them made very low intelligence test scores (75 and 96, that is, in the lowest 10% of the Main Group). These data are supported by observation on performance in the maze, which indicated a difficulty in comprehension of task, and a marked trial and error procedure. Both were diligent plodders. One moved very rapidly and the other very slowly.

The other 2 *S*s found difficulty in adopting a method of procedure. One case may be described as failure due to pronounced pressure at the beginning of the maze, very rapid movement, poor coördination, and poor motor control. The other case although displaying a better performance failed to adopt a uniform

method of tracing through the maze.[26] This *S* seemed to have difficulty in utilizing side cues. The performance varied between following the inner and outer sides of the paths. Culs-de-sac were eliminated early in the performance.

Summary

(1) This investigation shows that the ability to solve a maze problem is consistently related to the ability to learn a maze problem.

(2) General intelligence, as measured by a group test, appears more significantly related to the ability to learn a maze problem than to the ability to solve a maze problem (adaptability).

(3) Chronological age is more consistently related to performance in maze adaptability than general intelligence. Success in maze adaptability as contrasted with failure appears to be favored by a low chronological age and a high intelligence test score. But the relative degree of success is influenced by a higher chronological age, and general intelligence is negligible.

(4) General intelligence, performance in maze adaptability, method of procedure, temperament and other personality traits appear as significant factors in the case study of failures in learning.

(5) The results of this study, on the 5 maze adaptability problems, stand in opposition to the earlier results obtained by Cox: (a) significant measures are lower (smaller time-scores); and (b) correlations with general intelligence are negligible, in contrast to relatively higher correlations. However, intercorrelations (among the 5 problems) in the present investigation agree very closely with those obtained by Cox.

[26]Success in learning seems to be dependent upon the formation of a plan for tracing through the maze or an expressed recognition of the nature of the maze pattern. Although no information was revealed as to a particular tracing, there appeared a marked uniformity of response. One method of tracing, which appeared easiest, was adopted by 68% of the *Ss*. J. A. Gengerelli (Principle of maxima and minima in animal learning, *J. Comp. Psychol.*, 11, 1930, 227) says, "The essence of the experimental findings is essentially this: an animal, faced with a situation susceptible of an indefinite number of solutions, tends to elect that one which involves the least distance."

BEHAVIORISM, PSYCHOANALYSIS, AND THE PSYCHOLOGY OF P-FUNCTION

By Carl M. White, Cornell University

Subjectivism, regarded as a theory which conceives an inner mental substance and an outer physical substance as partners in a metaphysical dualism, has in recent years fallen into all but universal disrepute; but the consequent readjustment of psychological theory and practice has not yet been completely effected. In the welter of discussion provoked by this circumstance, psychoanalysis and behaviorism emerge, not necessarily as antithetical, but as extreme positions to the right and left. It now begins to appear that a central position, which should avoid the older metaphysical view and also escape the partialities of the newer extremes, would afford the psychology of our times a very great advantage. In his psychology of the P-functions Bentley has attempted such a central position. Let us examine this position and compare it right and left with behaviorism and psychoanalysis.

The functional position outlined in *Psychologies of 1930* found a partial expression as far back as 1924 in *The Field of Psychology*. But this treatise, classic though it is in certain methodological features, falters between structuralism, the best elements of which it sought to conserve, and a consistent functionalism. "The Composition of Experience" (Part I of the *Field*) analyzes 'experience' into the usual 'elements,' sensation, sensimages, and affective qualities; while "The Organization of Experience" attempts, with doubtful success, to regard these constituent 'elements' as they stand integrated. It is, therefore, not until we come to "The Psychosomatic Functions" that we reach the expected functional treatment. Perceiving, remembering and imagining, acting and feeling, understanding and thinking, are there described as modes-in-which-the-organism-acts, operates or performs, each mode differing from the others in certain typical aspects which may be observed and described. "Socialization" and "Development of the Psychological Organism" deal with the progressive development of these functions, which are conditioned at any moment of their history by stock, growth, physico-chemical

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exchange, association with other organisms, human events, and social traditions.

This treatment of the facts of psychology obviously makes the mind (as experientially defined) a co-partner with the physical structures in the *psychosome*; and it is the functional unity of this psychosome, or mind-body, which lies behind the psychosomatic functions. Here is more than a remnant of structuralism. More than a remnant also of the various functionalisms, cognitive and biological, which have been filling our psychological shelves since Stout and Angell wrote in the '90s. It is to the elimination of these substantively different partners that the author of the *Field* addresses himself in *The Psychologies of 1930*. Here the key-note is *function*. It is function of a psychological sort (P-function, in abbreviation) which supplies all the fundamental material for psychological studies of the living organism. No mental category is employed for supplying a mental substrate or 'structure' for the P-functions. There is no longer mind-body or psychosome. There is no longer substantive coöperation. The only 'structures' to be found in the organism are found by the anatomist and the morphologist. These furnish the common ground for all functions, albeit they are to be utterly stript of every materialistic or other philosophical connotation which would tend to prejudge or to limit their functional competency.

This departure from the *Field* involves, as a corollary, a revised view of the natural relations of psychological to biological studies. The new view sets these studies in a functional coördination. The biological functions (B-functions) are of the physiological sort as regards the internal economy of the organism and of the adaptive sort as regards the active relation of the organism to its environment. The concept of 'the environment' is handed over to the biologist and quite removed, as a basal term, from the field of the psychologist.

Now it should be profitable to compare for likenesses as well as for dissimilarities this revised view of psychology—which makes function basal, distinguishes psychology from the biological sciences, rejects the 'mental' both as content and as cause, and nevertheless preserves all of the usual problems and procedures of psychology—with that form of behaviorism which has proposed to limit itself to physical, chemical, and biological terms and concepts. We take, for the purposes of our comparison, the writings of Watson.

Both Watson and Bentley have agreed that the 'traditional' views of the task of the psychologist are unsatisfactory. For Watson,

this task is the description and explanation of behavior. With the completion of the behaviorist's program (which cannot, to be sure, be realized until his methods are improved) no really essential problem of introspective psychology will remain unsolved.[1] On the other hand, to impose upon psychology the task of transliterating behavior into terms of consciousness is, in any event, to proceed by analogy. It would mean that, after the behavior has been described, the conscious content of the animal or human subject would have to be *constructed*. Obviously, this involves an inference, and since an experimental science has nothing to do with inferences, the analogical procedure is absurd if psychology is to be a science.

In the second place, Watson and Bentley are more nearly agreed than might at first sight appear as to what constitutes the subject-matter of psychology. We have seen that that subject-matter consists for Bentley in certain distinctive functions of the total organism. The same is, in theory, true of Watson, who objects strenuously to the appellation, "muscle physiologist,"[2] and declares that "the behaviorist is interested in the way the whole body works," in "total activities," not in "muscle twitches."[3] In this connection, the outline of his *Psychology from the Standpoint of a Behaviorist* is illuminating. After the opening chapters, which deal with orientation and methods, the author describes, in the three following chapters, the mechanisms of behavior. If we are to credit his insistence that his program is different from the physiologist's, all of these chapters must be regarded as strictly preliminary. The rest of the book is devoted to a consideration of the functions of the 'whole organism.' Successive chapters treat of hereditary and acquired "modes of response," of the "organism at work" (turning from the acquisition of these modes to their employment by the adult individual), and of "the personality and its disturbance" (considering "how well or how poorly" the individual functions "in his present environment").[4]

Here we find a difference which is fundamental to an understanding of the sense in which either writer may legitimately be referred to as a functionalist. Watson means by 'function' either a mode of operation or a kind of achievement. Thus, when he speaks of function as a way in which the whole organism works, he is using the term in

[1] J. B. Watson, Psychology as the behaviorist views it, *Psychol. Rev.*, 20, 1913, 158-177.

[2] Watson, *Behaviorism* (rev. ed.), 1930, 15.

[3] *Ibid.*, 48. Cf. his *Psychology from the Standpoint of a Behaviorist*, 1919, 40-41 (hereafter referred to as *Standpoint*).

[4] *Standpoint*, 392.

the former sense; but when he speaks of function as a product of this way-of-working, as an adjustment or adaptation, he is using it in the second sense. For Watson, the adaptive signification is primary; but precisely the opposite is true for Bentley, whose P-function always denotes a mode of performance.

Once more, a fundamental difference arises over the criterion of objectivity. Watson defines objectivity epistemologically, *i.e.* on the basis of the traditional dualism of subject and object, of mind and body. By definition, therefore, all things 'mental' are subjective, while that is objective which is 'outside' of the observer in such a way as to be perceptible only to a bystander or onlooker. Thus, at one stroke, psychology is limited to those problems which may be envisaged from the bystander's system of reference. Bentley, on the other hand, defines objectivity in terms of method. It follows that there is for him no point in categorically denying the possibility of solving any problem that may suggest itself, because the solution will, in any particular instance, depend upon the ingenuity of the investigator, who must discover a type of procedure that will enable him to check his results experimentally at each step. A fact is objective, not with reference to a spatial analogy between an inner and an outer, but rather when manipulation of conditions reveals that (to use a figure) the universe or 'nature,' instead of the experimenter, has spoken thus-and-so. In actual practice, science knows no other form of objectivity than the methodological, a feature that enables the psychologist to test any theoretical issue that may present itself, even the behaviorist's tacit assumption that scientific laws can be formulated concerning only those phenomena which can be observed from the bystander's system of reference. When this deliverance is put to experience in the form of a question, we need go no farther than, say, the third law of color-mixture to ascertain that nature's deliverance does not coincide with the behaviorist's.

Another moment enters into the appropriation of the bystander's system of reference. In his early article on "Psychology as the behaviorist views it," Watson's point of departure is the fact that human psychology has not made good its claim as a *natural* science. This failure, we are told, is due to a mistaken conception of its subject-matter. It follows that the remedy must come through revising this mistaken conception; and behaviorism prescribes that the traditional subject-matter of psychology shall be conceived in precisely the same way as the sciences of nature (physics, chemistry, and the rest) con-

ceive theirs. "This suggested elimination of states of consciousness as proper objects of investigation in themselves will remove the barrier from psychology which exists between it and the other sciences. The findings of psychology . . . lend themselves to explanation in physico-chemical terms."[5]

Two questions naturally arise in this connection. First, if it is futile to construe behavior in terms of consciousness, as most psychologists would now agree, why is it any less futile to construe it in terms of electrical and chemical processes?[6] Obviously, the procedure is based on analogy in one case no less than in the other. The fact is, Watson evidently believes that the final test of scientific respectability, the "true explanation," of any biological or psychological phenomenon requires that its explanation be "couched in physico-chemical terms."[7] This brings us to a second question. Does the category of one science provide a more "true," or more nearly "true," explanation than that of another? Perhaps, if we care to give a metaphysical turn to the term 'explanation,' but if we remain on descriptive grounds, an affirmative answer is hardly plausible for the simple reason that the question itself is virtually meaningless. The purpose of the sciences is to describe, each from its own point of view, a common world. Scientific procedure is thus essentially selective, and the very excuse for the existence of more than one science lies in the fact that nature's several dimensions burst the mold of any single descriptive category. This being the case, it is nonsense to discuss degrees of trueness.

But apparently Watson is not to be deterred by the warning that questions of truth and reality should be left to metaphysics. A less militant protagonist of the bystander's system of reference would doubtless rest content with the simple declaration that the point of view is adopted and adhered to because it is believed to be the most fruitful approach to the study of psychological subject-matter. But Watson dutifully takes a critic to task for saying that the behaviorist merely ignores, but does not deny, the existence of what traditional psychology called 'mental states.' This is, he reproves, "a serious misunderstanding of the behavioristic position."[8] Passages like this plainly indicate that the behaviorist intends to cast his lot professionally with the materialistic metaphysicians. Whether this type of

[5] *Op. cit.*, footnote 1.
[6] Cf. *Behaviorism* (rev. ed.), 210.
[7] *Ibid.*
[8] Watson, Is thinking merely the action of language mechanisms? *Brit. J. Psychol.*, 11, 1920, 87-104.

Weltanschauung is defensible does not in the least concern us here; but this dabbling in metaphysics sets Watson in sharp contrast with Bentley, who draws a clear line of demarcation between scientific and speculative thought, and carefully consigns the problem of the ultimate nature of truth and reality to the latter.

In *Psychologies of 1925*, Bentley urged that, in preparing to treat scientifically a body of facts, the nature of the facts themselves should receive first consideration. Instead of being thrust into a preconceived pigeon-hole, these facts, if they are to be understood, must be described "in such subdivisions or aspects" as their own nature suggests. It is for this reason that Bentley refrains from analyzing the P-functions into 'simples' of any kind; for, if psychologists are, after all, dealing with modes of operation, it is their primary task "to define and to depict" these functional modes as such.[9] Just as one may acquire considerable information about the products of a printing press, their value, etc., and still be crassly ignorant of the way in which the press works, so we may acquire considerable information about the accomplishment of the organism, the worth of this accomplishment from the standpoint of environmental adjustment, and the like, and still remain comparatively ignorant of the going-on-to-accomplishment.

While, as has been suggested, behavior for Watson may, in a certain broad sense, be equated with the way the whole organism works, behavior, in his hands, nevertheless acquires meanings distinctly foreign to Bentley's P-function. As a preliminary step toward pinning down what is signified by the behaviorist's 'behavior,' it is necessary to distinguish at the outset between the behaviorist's faith and the behaviorist's practice. McDougall characterizes Watson as a *strict* behaviorist who "believes that two very simple conceptions suffice for the explanation of every instance of behavior, namely, the reflex and the conditioned reflex.[10] The characterization is apt so far as it goes; but this article of faith is no more important than the belief that the "true explanation" of every instance of behavior must be couched in physico-chemical terms.

Watson bases much of his actual procedure upon units of description which are 'simples' selected from some ontogenetic or phylogenetic source. "In order to understand man you have to understand the life history of his activities."[11] Tracing the "activity

[9]M. Bentley, *Psychologies of 1930*, 1930, 95-114.
[10]Wm. McDougall, *Psychologies of 1925*, 1926, 273-291.
[11]Watson, *Psychologies of 1925*, 1-35.

stream" back to its ontogenetic source in the human infant, Watson ends his quest for a 'simple' by adopting the more or less discrete reflex movement as ultimate. Accordingly, the congenital reflexes become the elements out of which all future behavior is compounded; behavior, in this sense, being nothing more nor less than such a compound. An interpretation of a world-wide economic situation and the Babinski reflex have fundamentally the same dimension; their only difference is a matter of complexity. The program of psychology differs from that of physiology only in the type of phenomenon reduced to this lowest common denominator. That program consists in noting movements and in analyzing them into their simple elements.

In so far as Watson is faithful to this principle of reduction, it is true *that* the discrete reflex serves as his basic category. But Watson the psychologist is not satisfied to limit himself to a program so obviously analogous to physiology, and to the extent that he introduces non-physiological features, of course, he abandons the discrete reflex movement as a category. Thus stimulus and response are both originally defined in physiological terms.[12] But, as he goes on, stimuli are introduced which only by the widest stretch of the imagination are describable in electrical, chemical, thermal and mechanical terms; he frankly expresses his fear that, after all, the reflex is an "over simplication."[13] He even concedes that, in its present state, physiology "cannot help us much" in a psychological description of behavior.[14]

Obviously, a new and more distinctively psychological category has been introduced, namely, environmental adjustment. That an 'adjustment' and a 'reflex' cite dimensions of experience that cannot easily be resolved into one another, without speaking loosely, will, I think, be disputed by few. Certainly no physiologist could be expected to be enthusiastic over the proposal to state the difference in my reactions to two situations as due to my witnessing a hunter in the one case "shooting squirrels" and in the other case "shooting rabbits."[15] But it is no less evident that Watson slips carelessly from one category to the other. "In this volume," he remarks in *Psychology from the Standpoint of a Behaviorist*, "the terms adjustment, response, and reaction are used almost interchangeably."[16]

[12]Vide *Standpoint*, 10-11.
[13]The fear is, however, suppressed in a parenthesis: *Behaviorism* (rev. ed.), 1930, 206-207. Cf. *Behaviorism*, 1924, 157.
[14]*Behaviorism* (rev. ed.), 210.
[15]*Standpoint*, 5.
[16]*Ibid.*, 12, footnote.

It would, of course, be a mistake to believe that the inconsistency has developed recently.[17] Watson is correct in holding that his has never been a muscle-twitch psychology; even though his contention is at most only a half-truth. In the work just mentioned, he indicates that "what we seek to have psychology busy herself with is just this matter of *environmental adjustment.*" The difference between psychology and physiology, in fact, lies precisely at this point; for "nowhere in physiology do we get the organism, as it were, put together again and tested in relation to its environment as a whole."[18] In short, the functional view of the Chicago School seems to have been embraced from the beginning: "In the main," he announces in 1913, "my desire . . . is to gain an accurate knowledge of adjustments and the stimuli calling them forth."[19]

This quotation brings us to the last important difference between Watson and Bentley. We may say that Bentley entertains the critical, Watson the Baconian, conception of science. Bentley takes seriously in his psychology the distinction between knowledge, appreciation, and use, contending that the psychologist must, if he is to *understand* his subject matter, adopt the knowing attitude.[20] Watson, on the other hand, states as the twofold aim of psychology: (1) the prediction of human activity with reasonable certainty; and (2) the formulation of laws and principles whereby man's actions can be controlled by organized society.[21] Although passages may easily be selected which fit awkwardly enough into such a view, the behaviorist's goal, as thus articulated, is inextricably linked with the discovery, by the manipulation of stimuli, of responses which are "in line with progress," "good," "desirable," and "appropriate."[22]

Such being his aim, Watson brings upon himself the necessity of treating his subject-matter causally. The control of human response depends upon a knowledge of the various types of stimuli which affect man and "of the places where such stimuli must be applied in order to produce appropriate action."[23] "Any object in the general environment"[24] may be a stimulus and its function as stimulus does

[17] Nevertheless, it does appear to be true that the psychological phase of the behaviorist's program comes into greater prominence in the revised edition of *Behaviorism* where Watsonian dogmatism is somewhat less rabid.
[18] *Standpoint*, 8 f., 20.
[19] *Vide* footnote 1.
[20] Bentley, *The Field of Psychology*, 1924, 13-16.
[21] *Standpoint*, 1-2.
[22] *Behaviorism* (rev. ed.), 20-47.
[23] *Standpoint*, 48.
[24] *Behaviorism*, 6.

not depend, theoretically, upon its relation to the organism; for causation is characteristically efficient, transitive, unilinear.[25] The stimulus incites, and then the organism moves. "The human is so built (and so are all other animals) that it *must move* when these stimuli *assail* it from within or without."[26]

We have here the causal schema of early English associationism minus a benevolent mind which manufactures ideas out of the sense material thrust upon it. As to the source of the conception, it should be noticed that, while Watson makes occasional reference to mechanics,[27] this science knows nothing of such a naïve view of causality. Unilinear causation, as a force which "produces," "calls out," "rouses," "excites," would work havoc with any sort of mathematical formula, because all content would be compressed into one side of the equation. Every cause is instead regarded as, in some sense, an effect; every force a function of relation which may be stated in terms of *differences* of velocity or work, and so on. The real source of the conception appears to be common sense, which conceives the relation of one thing to another after the analogy of certain functions of the experiencing individual. The vicious circle traversed by Watson accordingly seems to be somewhat as follows. Given a body of functions exhibiting a certain distinctive character known as agency, an anthropomorphic view of causality is derived therefrom, which, when it is projected in turn into the environment and used as an explanatory principle, obliterates the distinctive character of the virgin subject-matter. At this juncture, Bentley's motive for the substitution, for psychological purposes of description, of context for environment—whether 'inner' or 'outer'—becomes apparent. He does not believe that the P-functions, like the behaviorist's stimuli, happen non-contextually as by divine fiat. Least of all does he believe that, if and when regarded causally, the organism is a closed system. But having no oracular utterance to deliver on the subject of causality, and

[25] According to the scholastics, who derived their conception from the Platonic-Aristotelian *αἰτία*, a cause was an original substance, while an effect was either an activity of this causative thing or another substance which came to be in consequence of such activity. According to the rationalists of the seventeenth century, cause and effect are motions, motion tending to be regarded in turn as substantial. While it is not here intended to attribute philosophical sophistication to Watson, it is interesting to note that he adheres mainly to the scholastic notion of cause, *i.e.* any object (in the sense of naïve epistemological realism) in the environment; and to the rationalistic conception of effect, *i.e.* some sort of motion that is reducible to a physico-chemical substance. Cf. in this connection, W. Windelband, *Geschichte der Philosophie*, 1892, 324-326.

[26] *Behaviorism*, 197 (italics mine).

[27] *Standpoint*, 392-394.

believing that it is not incumbent upon the sciences to provide a metaphysical *explanation* of their subject-matter, he rests content with the more modest goal of *description*.

Where behaviorism inclines toward the right, psychoanalysis inclines toward the left of the psychology of P-function. Elsewhere the author has subjected psychoanalysis to more extended analysis,[28] pointing out that the practical motive, which is historically responsible for Freud's speculative approach to the meaning of the neurotic symptom, accounts primarily for the introduction of features which mar psychoanalysis as a psychological system. While the analysis was mainly critical, the constructive suggestion was offered that, since psychoanalysis deals with ways in which symptoms evolve in the life-history of the patient, these phenomena are primarily functions and functional products. Unnecessary difficulties might therefore be avoided by the elimination of the substantive and causal features. If these functions were grounded in the total organism instead of a doubtful mental substance, and if description supplanted causal explanation, psychoanalysis would swing decidedly in the direction of the psychology of P-function.

While Watson, as we have seen, proceeds in close analogy with physiology, Freud ignores physiology as irrelevant to his purpose. But like behaviorism, psychoanalysis is interested in practical control; and so the treatment here, as there, necessitates causal explanation, but in terms of mental substance instead of physical substance.

Psychoanalysis arose as "a method of treating nervous patients medically."[29] The peculiarity of several neuroses is that no corresponding organic lesions have as yet been laid bare, the correction of which will bring about recovery. "There are either no discoverable corresponding changes of the anatomical organ of the soul," Freud declares, "or else the changes are of such a nature as to yield no enlightment."[30] On account of this sterility of the physiological approach, the symptom is diagnosed and treated psychologically. "We shall entirely ignore the fact that the psychic apparatus with which we are here dealing is also familiar to us as an anatomical specimen. . . . We shall remain on psychological ground."[31] On this basis, a therapeutic program has been inaugurated, the clinical success of which confirms

[28]The categories of substance, cause and function in Freud's psychology, *Psychol. Rev.*, 39, 1932 (forthcoming).

[29]S. Freud, *A General Introduction to Psychoanalysis* (G. S. Hall, tr.), 1920, 1.

[30]*Ibid.*, 6.

[31]Freud, *The Interpretation of Dreams* (A. A. Brill, tr.), 1913, 424-425.

the contention that scientific laws can be formulated successfully from what may be called the experiencer's as opposed to the bystander's standpoint.

While in this respect psychoanalysis resembles the psychology of P-function, a difference between the two appears in the causal etiology of the former. In so far as Freud's genetic account of the symptom is descriptive, it differs in no essential detail from the genetic account of function given in *The Field of Psychology*.[32] But Freud, as has been suggested, is not content to stop with anything short of causal explanation; and his handling of the concept of causality is little, if any, more satisfactory than Watson's. Vitalism and mechanism are left side by side without any creditable attempt to remove the discrepancy between them and this despite the question as to whether the psychologist need involve himself in philosophical discussions of this nature. In fact, the negative importance of causal explanation for etiological purposes as illustrated by this inconsistency on the part of Freud appears to cast grave suspicion upon the line of argument designed to show that the scientist must provide an explanation of the subject-matter he is to describe.

The nearest we can come to a unit of explanation for Freud is unconscious idea or force. In rebelling against traditional psychology, Freud seemingly makes the common mistake of failing to replace the foundation of the view against which he launches his attack. Traditional psychology had held that psychological activity and consciousness are coextensive; that psychological subject-matter is homogeneous in kind; and that mind differs in kind from body. Being convinced, by his study of neurotic symptoms in their genetic setting, of the rôle of psychological activities which have no experiential coloring, Freud virtually equates psychological activity and the "unconscious" ("consciousness" being a sort of "sense organ" only); but the rest of the foundation is hardly shaken, except that his terminology threatens, at times, the homogeneity of his subject-matter.

As regards method, psychoanalysis is decidedly inferior to the psychology of the P-functions and to behaviorism, both of which, as we have seen, insist on high standards of experimental control. Psychoanalysis proceeds by clinical divination, and explains in terms of an assumed mental substance. The practical utility of this method when used as a therapeutic expedient may readily be conceded; but

[32] *Field*, 491-509.

until it is brought to a more empirical level psychoanalysis can scarcely—as its defender Flügel concedes[33]—claim recognition as a science.

By way of summary, we may say that the psychology of the P-functions has developed a thoroughgoing functional point of view, cutting free both from the structural type of analysis and from those compromising kinds of functionalism which have been drawn either from a teleological biology of adaptive ends or from the logic of cognition. This psychology of the P-functions differs, on the one side, from behaviorism in its system of reference and in its fundamental treatment of perceiving, remembering, acting, understanding, thinking, and the like, and, on the other side, from psychoanalysis in its expurgation of mental substances and causes. If differs from both in avoiding problems of ontology as well as in aiming at description for scientific purposes instead of at prediction and control for practical ends.

[33] *Psychologies of 1930*, 374-394.

INTENSITY AND NUMBER OF CONES IN FOVEAL STIMULATION

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For a long time it has been known that when a retinal image is of large area the intensity of illumination required to produce threshold stimulation is not so great as when the image is small. The brightness required varies in an inverse manner with the area—the larger the area, the lower the intensity; and the smaller the area, the higher the intensity. Despite the knowledge of this general relationship and the popularity of the problem, few hypotheses have been put forward as promising a valid explanation until the recent work of Adrian and Matthews.[1]

Much of the early work was deficient in many details. The most notable of the defects was the failure to provide a large enough range of areas of the stimulus-objects used. Because of this fact, any of the laws having to do with the reciprocal relation between intensity and area are applicable over a rather limited range of retinal surface. The work of Piper provides us with an excellent example of this shortcoming.[2] Piper's law—to the effect that the product of the square root of the area and intensity is a constant—was derived from experiments on the periphery in which the areas of the stimulus objects covered the limited range of 1–100 sq. cm. The law of Abney and Watson[3] (*Intensity* × *Diameter* = *Constant*) was obtained with a range which was even smaller than that of Piper. Piéron, as the result of a series of studies, has come to the conclusion that no general equation can satisfy the relation over a large range of areas.[4]

Reeves has performed one of the few experiments which may be considered adequate.[5] His range was large, the areas of the stimulus-surfaces varying from 0.75–14,400 sq. mm. One of the authors of this paper, as a preliminary to more extended investigation, attempted to give a quantitative formulation to Reeves' results. He found that the equation $AI^{1.3} = Constant$ holds over the middle range

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[1]E. D. Adrian and R. Matthews, The action of light on the eye, *J. Physiol.*, 63, 1927, 378 ff.; 64, 1927, 279 ff.; 65, 1928, 273 ff.

[2]H. Piper, Über die Abhängigkeit des Reizwertes leuchtender Objekte von ihren Flächen bezw. Winkelgrösse, *Zsch. f. Psychol.*, 32, 1903, 98.

[3]W. deW. Abney and W. Watson, The threshold of vision for different colored lights, *Philos. Trans.*, 216, 1916, 91.

[4]H. Piéron, La sommation superficielle des impressions lumineuses au niveau de la fovea, *C. r. Soc. Biol.*, 101, 1929, 1104.

[5]P. Reeves, The effect of size of stimulus and exposure-time on retinal threshold, *Astrophys. J.*, 47, 1918, 141.

of areas, but does not describe the relationship with the small and large stimulus surfaces.[6]

The researches of Adrian and Matthews provide the first step toward a satisfactory analysis of the problem. Such an analysis will be of immediate importance for our understanding of both photochemical and neural activity during light stimulation, and of general importance in so far as it contributes to our knowledge concerning the nature of synaptic transmission.

In an experiment involving the measurement of action currents from the optic nerve of the eel, Adrian and Matthews found that when the area of the stimulus-object was changed without varying the illumination there was a reduction in the latent period of the discharge, a quicker rise to the maximum and an increase in frequency which was not nearly proportional to the area stimulated. "It is obvious that these facts do not agree with the simple conception of the retina as a mosaic of sensitive points each connected with its own nerve fibre and each acting independently of the rest. The shortening of the latent period when the size of the retinal image is increased can only be explained on the assumption of some kind of interaction between the different stimulated points."[7]

Later experimentation has demonstrated that the interaction takes place in the nervous interconnections of the synaptic layers of the retina. This conclusion is a result of analyses made of rhythmic discharges found in the optic nerve of the eel when the entire retina is stimulated for a long time. Under such conditions of illumination the action current discharge loses its irregular character and consists of a series of regular waves at frequencies ranging from 5–15 a sec. This process is not due to rhythmical stimulation of the retina for when strychnine is applied to preparations which do not normally show the intermittent discharge, the synchronous activity appears at once.[8] If the process were photochemical we should suppose that strychnine would have no effect. Again, if four points on the retina are illuminated simultaneously, the reaction-time of the discharge is shorter than when any one of the points is stimulated alone. Strychnine causes a tremendous increase in the range over which this interaction occurs.

It seems safe to assume that the interaction process is a summation effect which takes place in the synaptic layers of the retina. Experiments of the type previously described leave no doubt as to the fact that the parallel effects of increase in area and increase in intensity are due to variations in the nervous summation process demonstrated.

Problem

The purpose of this paper is to make an analysis of the relationship existing between the threshold intensity of illumination and the size of area stimulated for the fovea of the human eye. Since determinations made in terms of area mean little (because of the manner

[6] C. H. Graham, The relation between area and intensity of visual thresholds, *Amer. J. Psychol.*, 42, 1930, 420. An error was made in this report which should be corrected. The retinal areas as given by the graph are all four logarithmic units too small. Every point on the curve should be raised that amount if the areas are to be read in sq. mm. This error does not, however, affect the validity of the equation. The only term changed by the transposition is the constant.

[7] Adrian and Matthews, *op. cit.*, *J. Physiol.*, 63, 1927, 378.

[8] *Idem.*, *J. Physiol.*, 65, 1928, 273.

of distribution of foveal elements), the investigation is extended for the purpose of making a correlation between *number of cones* available for stimulation and *intensity* of illumination. It is hoped that such a procedure will give a more accurate picture of the fundamental phenomena investigated than has been the case when *area* has been the variable. In order to make the analysis as complete as possible, the areas used have been chosen to cover a range varying from one which forms a retinal image covering as few as 12 to 16 cones to one which forms an image involving nearly the whole extent of the fovea.

Method

Apparatus. Our apparatus provides facilities for experimental precautions which have been overlooked by previous investigators of the problem, none of whom thought it necessary to provide a uniform starting point for the dark-adaptation process. The *S*s were allowed to stay in the dark for varying intervals up to the time when the threshold was determined. In no case was there a previous light-adaptation period.

Accurate and uniform results are obtained only when there is a uniform starting point and a uniform determination point. The uniform starting point can be accepted as the time when the eye is completely light-adapted. The determination point is taken as the time when the fovea is completely dark-adapted. The apparatus of the present experiment provides us with means of controlling this phase of the work.

Observations were made in a dark room. Light-adaptation was ensured by having *S*s look from a definite distance at a light-adapting surface for a uniform period of time. The dark-adaptation process was initiated by switching off the surface illumination. After a definite period of dark-adaptation *S* moved his head, placed his eyes at the observation openings, and was ready for a measurement. The threshold-determining objects were white circles of varying area, a different area for each determination. *S* was guided in his fixation by two small spots of light (luminous paint) which were at a horizontal distance of 25 cm. from the threshold object, the latter being in a position halfway between the spots. The areas of the circles were such that the range of visual angles subtended varied from 2.4′ to 3°.

The apparatus was, except for the stimulus-surfaces and the orientation spots, exactly the same as that used by Graham in an investigation of foveal binocular summation.[9] It consists of three principal parts: (1) A light-adapting screen having a brightness of 115 millilamberts. This is placed in a large, wall-board screen. In the screen are the observation openings which are fitted with artificial pupils of 4 mm. diam. (2) A threshold-determining object—a circle of white light—the area of which can be varied by means of cardboard diaphragms of different areas. These diaphragms are slipped into a groove in front of the milk-glass plate through which the light from the threshold-determining lamp passes. In the groove, behind the diaphragm, but in front of the milk-glass

[9] C. H. Graham, An investigation of binocular summation: I. The fovea, *J. Gen. Psychol.*, 3, 1930, 494 ff.

plate, is a holder into which neutral tint filters (Wratten) of varying densities may be slipped. These are used for gross adjustments of intensity. The threshold disk is at a distance of 75 cm. from *S*'s eyes. (3) A long, blackened box containing a 25-watt concentrated-filament bulb. (This lamp is behind the stimulus-object which is in the front part of the box.) Variations in the intensity of illumination of the white stimulus-circles are made by moving the lamp away from or toward the front of the box. Intensities are determined from a measured reading on the basis of the inverse square law.

Some slight modifications were made in the apparatus of the previous experiment. A buzzer was provided as a signalling device for *S*'s reaction to the threshold stimulus. When the *S* saw the white circle he pressed the key controlling the buzzer, and at the sound *E* switched off the threshold light. Artificial pupils of 4 mm. diam. were placed in the observation openings. A slide which could be adjusted by *S* (at the command of *E*) was used to cut off vision through either of the pupils. In practice *E* gave the order as to which of the pupils was to be covered.

Procedure. The *S* was seated in a chair (which was placed in a fixed position), with his eyes on a level with the artificial pupils and at a distance of about 35 cm. from the light-adapting surface which was slightly above them. Two minutes were allowed for light-adaptation. (At this intensity —115 millilamberts—light-adaptation is complete in this time.) During this interval *E* made the necessary adjustments of the apparatus, *i.e.* the slide determining the observing eye was set, the neutral tint filter was placed in its holder, and the diaphragm was slipped into the groove. At the end of the light-adapting period the lights were turned off and the dark-adaptation process was initiated. A period of 3 min. was allowed for dark-adaptation (enough for the fovea). A warning signal was given 10 sec. before the end of this period, and the determination of the threshold was made at the end of the third minute.

A movement of about 15 cm. was enough to bring *S*'s head to the chin-rest and artificial pupils. He had been instructed to fixate the center of the area between the two small luminous paint spots and to press on the buzzer key when he saw a white *circle*. The criterion of form was used in order to meet the requirements of foveal perception.

When the determination was to be made *E* gave the signal, switched on the 25-watt light and moved the carriage toward the *S* at a rate of about 10 cm. per sec. When the buzzer sounded the carriage was stopped, the stimulus light was switched off, the distance on the scale was read and the carriage moved back. The distance on the meter stick was recorded.

After the determination had been made, the *S* moved the slide so as to allow observation with the other eye, and within 30 sec. another determination was made. With the completion of the threshold measurements for the two eyes the experimenter switched on the light-adapting lights and the same procedure was repeated with a diaphragm of different area. All timing operations were made by means of a stop-watch. Four determinations were made with every *S* for each area of diaphragm—two for the left eye and two for the right.

The preliminary work occupied more time than was required by the experiment reported here. Results were obtained under several types of stimulating conditions. Among these were some which involved threshold determinations for

red light (Wratten filter No. 70) with and without artificial pupils, and some in which the stimulus-patches of white light were observed without artificial pupils. In these latter series a greater number of areas were used in the middle ranges than is the case in the main experiment, but the trend of the results was found to be so regular that only 12 disks of different area were used in the final measurements. In addition to these variations in procedure, we tested for the effects of differing periods of light- and dark-adaptation, and for the influences dependent on the spacing of the luminous paint spots. Our experience in the early experiments has led us to believe that the procedure finally adopted is one which approaches the optimum.

Results

Area and intensity. Measurements were made with 5 *S*s. The averaged data are presented in Table I and in Fig. 1. The relation expressed by these two methods is so similar to the relation given by every *S* individually that we are content to reproduce only the group averages. The table and graph give an expression of the way in which the threshold intensity decreases as the area of the retinal image increases.[10] With the artificial pupils in use the pupillary area was 12.57 sq. mm. for all *S*s. Retinal areas were calculated on the assumption that a visual angle of 1′ subtends a retinal distance of 0.005 mm.

A survey of Table I and of Fig. 1 makes it clear that as the stimulus-area increases in size the threshold intensity decreases.

TABLE I

Relation Between Area and Intensity of White Light Thresholds

Stimulus area	Diameter (cm.)	Visual angle	Retinal area (sq. mm.)	Intensity (photons)
1	0.054	2.52′	0.0001131	1.96
2	0.07	3.24′	0.0001985	1.23
3	0.1	4.56′	0.0004085	1.19
4	0.125	5.76′	0.0006396	0.339
5	0.14	6.36′	0.0008050	0.239
6	0.162	7.20′	0.001087	0.194
7	0.2	9.12′	0.001630	0.104
8	0.5	22.68′	0.01031	0.056
9	1.0	48.08′	0.04126	0.012
10	2.0	91.56′	0.16503	0.0093
11	3.0	138.00′	0.37133	0.0064
12	3.8	172.80′	0.59529	0.0071

[10]The intensities are expressed in terms of photons, a physiological unit of intensity proposed by Troland which makes allowance for variations in pupil size (cf. L. T. Troland, The measurement of visual stimulation intensities, *J. Exper. Psychol.*, 2, 1917, 1-24). Hecht has modified Troland's definition and regards the photon as the retinal illumination produced when the eye looks at a brightness of 1 millilambert through a pupil of 1 sq. mm. (cf. S. Hecht, The relation between visual acuity and illumination, *J. Gen. Physiol.*, 11, 1928, 261).

The relationship, however, is not regular. Between areas 3 and 4 a distinct break is to be noted in the intensity values. This means, of course, that the small areas (1, 2, 3) require an increase in intensity which is out of proportion to the decrease in area. Considerations concerning the possible significance of this break in the curve will be taken up later. A similar break in the intensity values occurs between areas 8 and 9. The results as obtained in the preliminary experiments without artificial pupils (those in which a greater number of

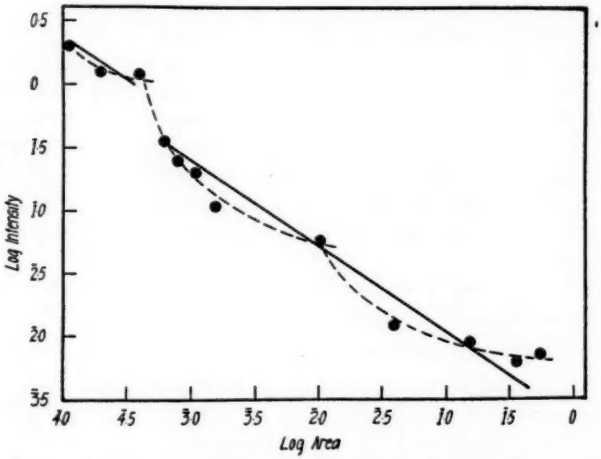

FIG. 1. RELATION BETWEEN THE SIZE OF THE STIMULUS-AREA AND THE INTENSITY THRESHOLD

intermediate determinations were made) seem to verify this observation. An interpretation of this discontinuity will be made later also.

The two solid straight lines drawn through the points of the curve indicate the fidelity with which the equation obtained with Reeves' data applies to the present measures. These lines are drawn so that any point satisfies the equation $AI^{1.3} = K$, (the value of K differing in each line) or approximately $A^{0.75} I = k$. (It makes little difference whether we consider area or intensity as the independent variable.) We may note that this equation provides only the crudest of empirical descriptions. The broken line through the data is a more accurate index of the true relationship.

The results as presented make it clear that an increase in the number of retinal elements available for stimulation has as its correlate a decrease in the amount of light energy per cone necessary for stimulation. The reciprocal relation is not a regular one. There are two discontinuities in the curve relating the two variables. These will be clearly distinguishable in the curves to be presented in a later section.

In considering the discontinuity in the curve which is found after area 3 the writers find it necessary to raise a point which centers around the following fact. When we work with stimulus-objects of small area it is certain that the size of the retinal areas stimulated are not those which geometric optics would indicate. This state of affairs is due to conditions of scatter and other optical defects. It might be argued that the break in the curve following area 3 is due to such factors. Such a hypothesis may be tested by plotting the product of area and intensity against the diameter. If the break still occurs, we must assume that scattered light cannot account for our results. The reason for such a graph is clear. If the light is scattered the stimulated area is larger than that dictated by geometrical optics, but the intensity per unit area is smaller. The product of area and intensity when plotted in the previously described manner should show no discontinuity if this source of error is a factor. When the results of the present experiment are treated in this manner it is found that the break still occurs after area 3.

We may mention that with red light (without artificial pupils) the equation $Intensity \times Diameter = C$ seems to hold over a wide range of the curve. The three smallest areas tested in the final series with white light were not, however, used in these early observations. The constant varies in value from 0.00019–0.00028. The preliminary experiments with white light (smallest areas eliminated, no artificial pupils) show that the same relation holds. In this case the constant varies between 0.00013 and 0.0002. Intensities are in terms of millilamberts. The diameters are the retinal diameters. Little significance is attached to these expressions because of the fact that it seems impossible to derive any meaning from them.

Intensity and number of cones. In order to arrive at a better understanding of our results we have extended our analysis to take into consideration the number of cones available for stimulation in each of the retinal areas tested. A formulation involving *area* and *intensity* means little, because of the fact that the cones are not distributed in such a manner that we can consider the number as directly proportional to the area. The population is less dense along the superior-inferior axis than along the temporal-nasal axis. Further than this, there is a definite decline in the number of cones per unit area as the distance from the center of the eye increases.

Determinations of the number of cones available for stimulation in each of the retinal areas tested were made on the basis of the ob-

servations of Rochon-Duvigneaud.[11] Photomicrographs of sections through the fovea comprise an important part of the work of this investigator. All of the photomicrographs are of vertical sections showing the different layers of the retina and give excellent length-

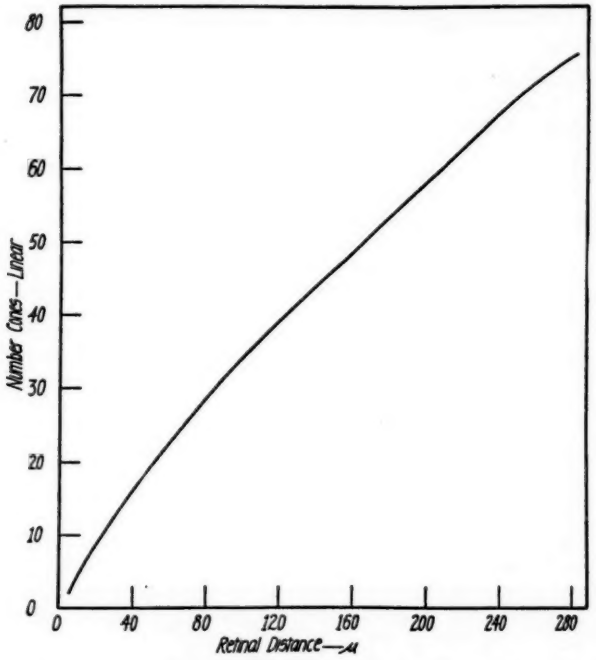

FIG. 2. RELATION BETWEEN THE NUMBER OF CONES AND THE DISTANCE FROM THE CENTER OF THE RETINA

wise views of the cones. Since the magnification is given, we were able, by careful measurement, to determine the *linear* number of cones contained in given *distances* from the center of the fovea. The measurements were carefully checked with those of Rochon-Duvigneaud with respect to width and height of cones. Because of the fact that the sections were cut only along the superior-inferior axis, we could not determine the average distribution along the four axes,

[11]Rochon-Duvigneaud, Recherches sur la fovea de la rétine humaine, *Arch. d'anat. microscop.*, 9, 1907, 315.

and so we present the data as the average distribution of cones for the superior and inferior axes only.

Fig. 2 gives a graphical presentation of the way in which the number of cones (linear) varies with the distance from the center of the retina. This graph was drawn on a large scale, and when the number of cones for a given radius of retinal area was determined we read off the number of elements corresponding to the retinal distance of the radius. This reading was checked by means of linear interpolations of the tabulated material. Results obtained by this method are given in Table II.

TABLE II

INTENSITY AND NUMBER OF CONES ALONG RADIUS OF RETINAL IMAGE

Stimulus area	No. Cones	Intensity (photons)
1	2	1.96
2	3	1.23
3	4	1.19
4	5	0.339
5	6	0.239
6	7	0.194
7	9	0.104
8	20	0.056
9	47	0.012
10	64	0.0093
11	85*	0.0064
12	96*	0.0071

*These two values were determined by extrapolation of the curve constructed from the data of Rochon-Duvigneaud.

Here are presented the number of cones lying along the radius of each of the retinal images given by the different sizes of diaphragm together with the corresponding threshold intensities. Fig. 3 gives a graphical picture of the relationship. The logarithmic graph is presented for the reason that the range of intensities encountered warrants such treatment. While it is true that the number of cones is not so great as to necessitate similar manipulation, it is certain that the data appear in a more balanced manner if the procedure is adopted in the treatment of the cones also.

In order to arrive at an approximation to the number of elements in the different retinal areas, the number along the radius was squared and multiplied by a constant which careful estimation determined as 4. This expression is equal to the square of the number lying along the diameter. The number as calculated in this manner

takes care of the fact that the number lying on the superior-inferior axis (the basis of our calculations) is smaller than the number lying on the temporal-nasal axis.

The relationship existing between the available number of cones and the threshold intensities is given in Fig. 4 which is a graph of *log I* as a function of *log Number Cones* (total). Figs. 3 and 4 show that the discontinuities found in Table I and Fig. 1 still exist in the present treatment.

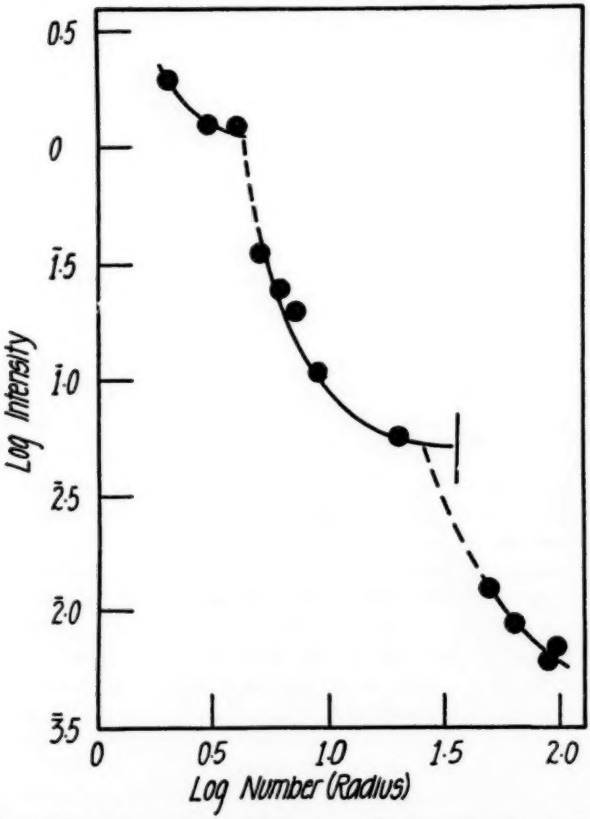

FIG. 3. RELATION BETWEEN THE INTENSITY THRESHOLD AND THE NUMBER OF CONES LYING ALONG THE RADIUS OF THE RETINAL IMAGE

Discussion

In discussing the results of this experiment we have consistently stressed the fact that the relationship between number of cones and intensity is a discontinuous one. The first break in the curve occurs when the retinal area stimulated increases from approximately 0.00041 sq. mm. to 0.00064 sq. mm. (64 to 100 cones). At this point there is found a precipitous decrease in the intensity necessary for stimulation.

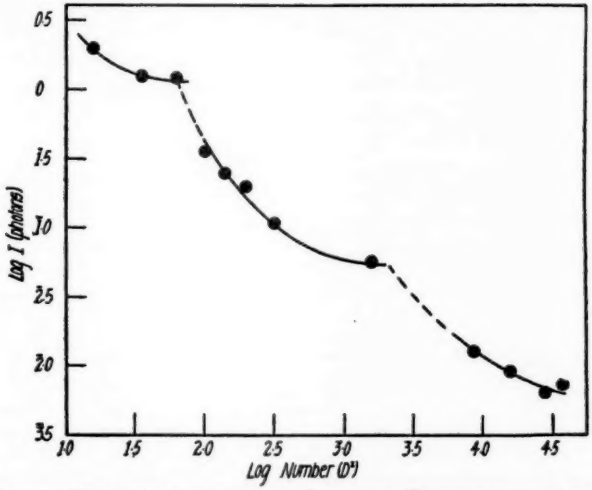

Fig. 4. Relation between the Intensity Threshold and the Total Number of Cones Available

The second abrupt change occurs when the number of cones increases from 1600 to 8836 (areas 0.01 sq. mm. to 0.041 sq. mm.). (This break was verified in the earlier series under conditions where a greater number of intermediate areas were presented to the subject.) We are led to believe that this latter change of slope is due to the presence of functioning rods. Such an interpretation is made plausible because of the fact that the diameter of the rod-free area has been determined as about 200μ (Rochon-Duvigneaud). The vertical line in Fig. 3 designates the approximate position on the curve where the rod-free area ends. It is to be noted that the position of this line and the break in the curve coincide very well. Because of the agreement we find it probable that the second discontinuity can be attributed to the presence of rods.

It is impossible to attribute the abrupt change after the third of the smallest areas to a similar anatomical feature of the fovea. The photomicrographs show that there is no topographical correlate for the sudden change in slope. We are left, then, with the conclusions that the discontinuity must have its basis in some functional process.

The data show that with very small retinal areas (making available for stimulation only a small number of cones) a disproportionately high light intensity is required to produce a threshold effect. When

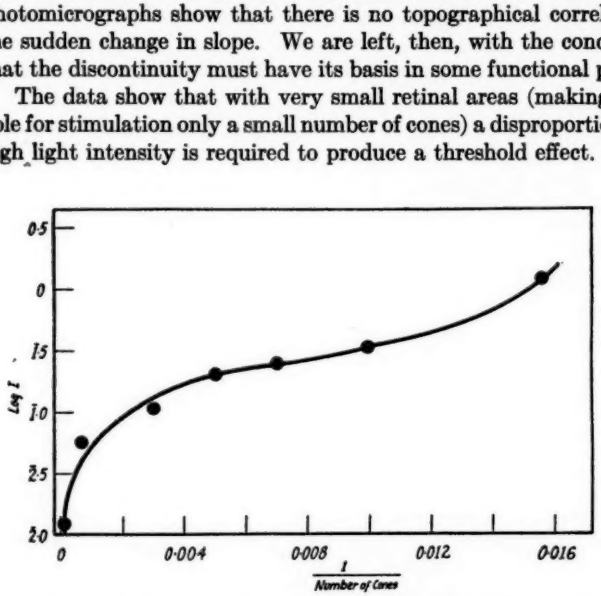

FIG. 5. RELATION BETWEEN LOG I AND THE RECIPROCAL OF THE NUMBER OF CONES AVAILABLE

(Because of the range of stimulus-areas used and the difficulty of representing all of them in one graph, the data from areas 1, 2, 10, 11, and 12 were not included in constructing this curve.)

the number of end-organs reaches a certain minimum (approximately 50–100) there is a decidedly lower light requirement. The only interpretation which can be made of such a disproportionate decrease in the light intensity is that there is coming into play a type of physiological process which requires a certain number of end-organs before its threshold of activity is reached. When the number of cones is below this threshold number, each element requires a high intensity for stimulation.

The form of the curve after the first break is approximately what one would expect from the work of Adrian and Matthews. As the area increases the threshold intensity of light decreases in a regular, hyperbolic manner until (if we judge from the form of the curve) an

increase of area ceases to affect the intensity. On the other hand, the behavior of the small areas is best described by an independent curve.

Since the relationship of area and intensity over a large range is not a simple one we must conclude that the total process initiating the nerve impulse is different with very small areas from what it is with larger areas. There can be little doubt that the sudden change in intensity as the number of cones increases from about 50 to 100 is due to the development of the summation process which takes place in the synaptic layers of the retina. The mechanism of the process is obscure. The results of this experiment point to the fact that with small numbers of end-organs there is little or no summation. As the number available for stimulation increases from approximately 50 to 100 the threshold for interaction (or at least for a more efficient interaction process) is reached.[12]

We are led to believe that our results cannot be interpreted on the basis of an alternative possibility, *i.e.* Hecht's concept of a statistical distribution of thresholds as affording an explanation for the area-intensity relation.[13] It would follow from the application of this idea that a plot of *log I* against the *reciprocal of Number of Cones Available* would give an approximately straight line with positive slope. Such a state of affairs follows from the assumption that the number of elements brought into play in a given region is approximately proportional to the logarithm of the intensity, and from the idea that it is the number activated which determines the threshold. Fig. 5, which shows the data of this experiment treated in this manner, demonstrates considerable deviation from this expectation. Although the test cannot be considered critical, the deviation from the expected relation leads us to examine our results in terms of interpretations dealing with synaptic reactions similar to those demonstrated by Granit and Harper[14] who have shown that reflex activities having summation and occlusion aspects play definite rôles in vision. Statistical distribution of thresholds is not excluded as a contributing factor, but it is difficult to see how it alone can explain the results.

Summary

A critical summary of some of the early work on the problem of the relation of area and intensity of visual thresholds has been presented. It is shown that in most instances the experimental work has been inadequate. There follows a brief description of the recent work

[12]It is necessary to point out that the possibility of a shift in fixation in the case of the small areas cannot account for the results. Such shifts are known to occur in dark-adaptation. However, such a shift would be in the direction of a retinal area of higher sensitivity and the threshold would be lower. Further, we believe that the presence of the luminous orientation spots would eliminate all tendencies in this direction.

[13]S. Hecht, *op. cit.*, 278.

[14]R. Granit and P. Harper, Comparative studies on the peripheral and central retina: II. Synaptic reactions in the eye, *Amer. J. Physiol.*, 95, 1930, 211-228.

of Adrian and Matthews on the measurement of action currents from the optic nerve of the eel, together with a discussion of the theoretical implications of the investigation.

By means of a method which has been described we attempted to determine the relationship existing between the intensity of the threshold stimulus and the area of the retina (predominantly the fovea) stimulated. The results as obtained make it clear that no simple relation exists between the two variables.

Nor is there a simple relation existing between *intensity* and *number of cones* for the areas tested. With small areas a disproportionately high light intensity is necessary for stimulation. When the area is such that it comprises 50 to 100 cones there is a sudden decrease in the intensity required. This change of relation is interpreted as signifying the arousal of an interaction process which requires a minimum number of end-organs (or nerve fibres) before it reaches its maximum efficiency.

Another discontinuity is found in the curve relating *number of cones* and *intensity* when the number increases from 1600 to 8836. We are led to believe that this change of relation is brought about by the presence of functioning rods. Determinations concerned with the limits of the rod-free area strengthen this supposition. It is found that the number of cones within the rod-free area and the number in the vicinity of this break are approximately the same.

AFFECTIVE CONTRAST BETWEEN MODALITIES

By Albert J. Harris, Purdue University

In a previous paper by the writer,[1] evidence was presented which showed that affective contrast between experiences of the same modality is of the nature of mass-contrast, adequately described by the law of affective equilibrium. This law is stated as follows: "The affective value of the experiential correlate of a stimulus varies conversely with the sum of the affective values of those experiences preceding this correlate which constitute with it a unitary temporal group."[2] From that evidence no prediction could be made whether affective contrast could occur between experiences of different modalities, or what the nature of such contrast, if found, might be. The experiments reported below were performed in an attempt to answer these questions.[3]

Experiment I

The apparatus consisted of (1) 90 white cards, 4 in. square, on which were mounted 1-in. squares of 90 different Milton Bradley colored papers; (2) 23 1-oz. bottles containing 5 cc. of 23 odorous liquids;[4] (3) the presentation-box—a large box with metal top and front, containing a glass turntable. Stimulus-bottles placed by *E* on the turntable, at the open back of the box, could be brought quickly around under a hole, in the top front, at which the *O* sniffed. Color cards

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[1] A. J. Harris, A study of affective contrast, this Journal, 41, 1929, 617-626.

[2] J. G. Beebe-Center, The law of affective equilibrium, *ibid.*, 41, 1929, 64.

[3] A more complete account is given in the writer's dissertation, *Studies of Affective Contrast*, on file in the Harvard University Library. The writer wishes to acknowledge his indebtedness to Dr. J. G. Beebe-Center, for suggestion of the problem and invaluable help throughout; and to Dr. E. G. Boring and Dr. H. H. Remmers for valuable criticism, especially on the statistical aspects of the work.

[4] The odorous liquids in approximate order of pleasantness, were:

Oil of jasmine	Oil of thyme
" " Ceylon cinnamon	" " petitgrain
" " sweet orange	" " patchouli
" " cloves	" " rosemary
" " carnation	" " rose geranium
" " bergamot	" " cumin
" " cananga	Denatured alcohol
" " juniper berries	Cod liver oil
" " anise	Butyl nitrate
" " nutmeg	Tincture of asafoetida
" " fennel	Valeric acid
	Carbon disulphide

The essential oils were obtained from Fritsche Bros., N.Y.C.

were exposed through an aperture in a gray cardbord screen, attached to the top of the box. The *Os* were Messrs. W. D. Turner (*Tu*), K. W. Oberlin (*Ob*), H. L. Kim (*Ki*), and Miss D. Selling (*Se*). All were experienced in making affective judgments.

In a preliminary series of judgments, the 9 most P (pleasant) and 9 most U (unpleasant) odors were selected for each *O*, giving individual P odor sets and U odor sets. On the basis of previous results, *E* selected for each *O* a set of 20 colors about evenly distributed over the scale from very P to very U, for that *O*. The experiment proper involved the presentation of the 20 colors alternating with 9 odors in one continuous series. A haphazard order of presentation was arranged in which each color followed each of the 9 odors once. The experiment was divided into two parts, in one of which P odors were used, and in the other, U odors. For *Tu* and *Se* the P set was used in Part I and the U set in Part II; for *Ob* and *Ki* the U set was given first. Each part was completed in 3 hours, spaced 1 week apart, with no extra interval between parts. Each hour was divided into 3 series of 20 colors and 20 odors each. The colors and odors were presented alternately, with an odor always first, and with an interval of approximately 15 sec. between each 2 stimuli. There was a rest of 3 min. between series.

The instructions were as follows:

> Odors are to be presented to you one at a time. You are to take two sniffs of each, and report whether the odor is very pleasant, pleasant, indifferent, unpleasant, or very unpleasant. After each odor a color will be presented. You are to judge the color in the same way as the odors. Try to judge each stimulus independently, *avoiding all memory or association effects.*

Protocols made at the end of each hour were recorded verbatim by *E*. In calculating results, the judgments were given numerical values as follows: very P, +2; P, +1; indifferent, 0; U, −1; and very U, −2.

Contrast might appear in this experiment in either of two ways. In the first place, each color might be more P after the more U odors of a given set than after the more P odors of the same set; this would be an individual contrast effect. In the second place, there might be a mass contrast effect, with the colors as a group, judged to be more P when intermingled with the U odors than when intermingled with the P odors.

Individual effect of the odors. As each color followed each of the 9 odors of a given set once, comparison of the average affective value of the 20 colors following one odor, with the average value of the same colors following another odor of the same set, will indicate any difference in effect of the two odors. Table I shows the values obtained when this is done. It is noticeable that the figures on the *right* side of each horizontal line of averages are, if anything, slightly higher than the averages on the left side of the same line in any case. If there were individual contrast, the averages on the *left*, following

the relatively U odors, should be considerably higher. Furthermore, when the total averages are considered, the uniformity of the 9 columns is striking. The average for the colors following the least P odors and that for the colors following the most P odors are equal. There is no indication that the individual odors of a given set had any differential effect upon the pleasantness of the colors. We may conclude, then, that there is no evidence of individual contrast in this experiment.

TABLE I

AVERAGE AFFECTIVE VALUES OF THE COLORS FOLLOWING THE INDIVIDUAL ODORS

	Odor	(Least P)		Rank Order of Odors					(Most P)	
O	Set	1	2	3	4	5	6	7	8	9
Tu	P	1.30	.85	1.55	.95	1.45	1.45	1.30	1.10	1.40
	U	.60	.45	.55	.50	.85	.70	.55	1.00	.80
Ob	P	.55	.30	.48	.48	.30	.30	.40	.35	.55
	U	.65	.25	.40	.55	.55	.65	.55	.43	.43
Ki	P	.80	.85	.85	1.10	.80	.65	.70	.60	.75
	U	.60	.60	.25	.35	.40	.40	.75	.45	.45
Se	P	.00	.30	.15	.20	.25	.10	.10	.15	.20
	U	.05	.20	.15	.00	−.05	.15	.00	.15	.00
Av.		.57	.48	.55	.52	.57	.55	.54	.53	.57

Mass effect of the odors. In Table II the total average affective value of the colors is shown for the two conditions, presented with P odors and with U odors. For 3 of the *O*s, the average is higher with P odors than with U odors. This type of effect—the opposite of contrast—we may call an assimilation effect. For the fourth *O*, there is a slight shift in the direction of contrast.

The question immediately arises whether these shifts are actually the result of the change in odors, or show merely chance variation. Two statistical methods are available for testing the probable significance of the shifts.

In one of these methods, which we may call Method X, the average affective value of the colors in each part of the experiment is considered as the mean of 9 values, which are the average affective values of the 9 successive presentations of the 20 colors. The standard error of each mean can then be found, and the standard error of the difference between the two means.[5]

[5] H. E. Garrett, *Statistics in Psychology and Education*, 1926, 128-33.

In the second method, Method Y, the shift is considered to be the difference between the number of colors that on the average became more P with a change in odors, and the number of colors that on the average became more U. The number of colors that on the average became more P with a change in odors, the number of those whose affective value did not change, and the number of those that on the average became more U, are given in columns 5, 6 and 7 of Table II. It is obvious that the greater the preponderance of changes in one direction over changes in the other is, the more significant it is. The relative significance of different degrees of preponderance can be stated statistically. The standard deviation of the proportion for a given category may be obtained by use of the formula:[6] $\sigma_{prop} = [(p.q.)/N]^{\frac{1}{2}}$.

When the standard deviations for the two categories (colors that became more P and colors that became less P) have been obtained, the standard error of the difference between the two categories (preponderance in one direction) may be found in the usual way.

In general, the difference between two measures may be considered maximally significant if it is 3 or more times as large as its standard error.[7] The results obtained by use of the two methods are shown in columns 4 and 8 of Table II. It is apparent that by both methods the shifts for *Tu* and *Ki* are statistically significant, and that by both methods the shifts for *Se* and *Ob* are of very doubtful significance. It will, of course, be noted that the standard errors are based on what would ordinarily be considered too small a number of cases.

TABLE II

SHIFTS IN THE AFFECTIVE VALUE OF THE COLORS WITH A CHANGE IN ODOR SETS: EXPERIMENT I

O	1 Color av. after P odors	2 Color av. after U odors	3 Shift in color av.	4 Method X $D/\sigma_{diff.}$	5 No. colors higher after P odors	6 No. colors with no change	7 No. colors higher after U odors	8 Method Y $D/\sigma_{diff.}$
Tu	1.25	.67	.58 a*	5.58	17	1	2	7.21
Ob	.40	.49	.09 c*	1.32	7	2	11	1.30
Ki	.79	.47	.32 a	3.45	15	3	2	5.04
Se	.17	.08	.09 a	2.78	9	5	6	.99

* 'a' indicates assimilative shift; 'c' indicates contrast shift.

That is an unavoidable result of the experimental procedure, and must be considered as a limitation upon the conclusions to be drawn.

[6] T. L. Kelley, *Statistical Method*, 1923, 86-90.
[7] Garrett, *ibid*, 134.

The protocols of the *Os* agree with the statistical conclusions. *Tu* reported, after an hour with the P odors, that a state of great exhilaration had been built up, and that, although he tried to avoid calling the colors more pleasant because of this general state, he did not know if he had succeeded. *Ki* reported, after an hour with U odors, that it was harder than usual to get pleasure from the colors. Other similar reports were made by these two *Os*. *Ob and Se* both reported that the odors had no effect upon their judgments of the colors.

Discussion. This experiment shows definitely that affective contrast is not the usual result of inter-modal affective stimulation. Of the 4 *Os*, only one gave any sign of contrast, and then the amount was so small as to be statistically doubtful. The experiment also shows that for some *Os* an assimilative effect may occur; colors will be judged to be more P when intermingled with P odors, and less P when intermingled with U odors. The influence in these cases seems to be the result of a general mood created by the odors.

The idea that moods may influence affective judgments is not new. The effect of a headache or toothache upon one's aesthetic appreciation is a commonplace of everyday experience. Experimental evidence on the question, however, is slight. Recently, P. T. Young has reported a case in which, during an experiment on the constancy of affective judgments, a marked drop in pleasantness occurred on a day when the *O* was in an unusually unpleasant frame of mind.[8] It is probable that strong moods, caused by factors not controlled by the experimental situation, would result in assimilative effects similar to those found in this experiment. The reports of the *Os*, however, definitely give the odors as the cause of the general states they describe, and the moods reported were always in accord with the type of odors presented.

The fact that no clear evidence of intermodal affective contrast was found in this experiment did not mean that such an effect would never occur. Special circumstances might be necessary which were not provided in this experiment. Accordingly, further experimentation was planned.

Experiment II

The apparatus was the same as in the preceding experiment. The *Os* were Messrs. Oberlin (*Ob*) and Kim (*Ki*) from the preceding experiment, and 6 new *Os*, Miss J. Steinberg (*St*) and Messrs. W. A. Hunt (*Hu*), M. H. Lewis (*Le*), J. D. Frank (*Fr*), E. J. Ludvigh (*Lu*) and M. Sherif (*Sh*), all research students in the Harvard laboratory. For *Ki* and *Ob* the same colors and odors were used as in Experiment I. For the other *Os*, individual P and U odor sets were selected as in Experiment I, after one hour of preliminary judgments on

[8] P. T. Young, Studies in affective psychology, this Journal, 42, 1930, 26.

odors alone. A set of 20 colors, used for all of these *O*s, was selected by *E*. The order of presentation of colors and odors was the same as in Experiment I.

The experiment had 3 parts. For *St*, *Hu*, *Le*, *Fr*, and *Lu*, the colors were presented intermingled with the U odors in the first part, then with the P odors, and finally with the U odors again. For *Sh*, *Ki*, and *Ob*, the P odors weregiven first, then the U odors, and finally the P odors. Each part consisted of 3 experimental hours, as before. The hours were spaced 1 week apart, with no extra interval between parts. The instructions were essentially the same as in Experiment I.

Individual effect of the odors. The average affective value of the 20 colors following each odor of a given set was calculated as in the preceding experiment. As before, the averages for all *O*s were practically the same, following the least P odors of a set, as they were following the most P odors of the same set. No traces of individual effect of the odors were found.

TABLE III

SHIFTS IN THE AFFECTIVE VALUE OF THE COLORS WITH A CHANGE IN ODOR SETS
EXPERIMENT II

O	Change of odors	1 Color av. after P odors	2 Color av. after U odors	3 Shift in color av.	4 Method X $D/\sigma_{diff.}$	5 No. colors higher after P odors	6 No. colors with no change	7 No. colors higher after U odors	8 Method Y $D/\sigma_{diff.}$
St	U-P	.56	.44	.12 a	3.58	8	8	4	1.42
	P-U	.56	.59	.03 a	1.14	4	11	5	.36
Hu	U-P	.41	.32	.09 a	.95	9	4	7	.65
	P-U	.41	.49	.98 c*	.75	6	5	9	.99
Le	U-P	.55	.47	.08 a	1.14	12	3	5	2.39
	P-U	.55	.67	.12 c	2.30	6	4	10	1.32
Fr	U-P	.25	.17	.08 a	1.31	11	4	5	2.04
	P-U	.25	.22	.03 a	.55	9	2	9	0.0
Lu	U-P	1.18	.53	.65 a	13.68	18	2	0	13.44
	P-U	1.18	.25	.93 a	16.52	20	0	0	—
Sh	P-U	.54	.86	.32 c	3.09	2	2	16	6.31
	U-P	.56	.86	.30 c	3.23	2	8	10	2.98
Ki	P-U	.92	.85	.07 a	.82	9	5	6	.99
	U-P	.87	.85	.92 a	.22	8	2	10	.64
Ob	P-U	.06	.57	.51 c	6.88	1	1	18	10.64
	U-P	.34	.57	.23 c	2.95	4	4	12	2.84
						129	65	126	

* 'a' indicates assimilative shift; 'c' indicates contrast shift.

Mass effect of the odors. The main differences between this experiment and the preceding one were the greater number of *Os* and the addition of a third part. It was hoped that this would give a better opportunity for individual differences to appear, if that were the solution of the problem, and also that the more extended experiment might offer a condition, necessary for the occurrence of contrast, that was not present in the first experiment.

The results for the 8 *Os* are summarized in Table III. The total color averages for each part are shown in columns 1 and 2. Column 3 shows the amount and direction of the average shift between each two parts. The directions in which the individual colors shifted are shown in columns 5, 6, and 7. The results obtained by applying Methods X and Y are shown in columns 4 and 8.

It will be noted that for *St*, *Hu*, *Le*, *Fr* and *Ki*, not one shift is indicated to be significant by both methods. The first shift for *St*, a small shift in the direction of assimilation, significant by Method X but not by Method Y, is statistically the most significant. However, it is no larger in amount than the second shift for *Le*, which is of doubtful significance by both methods. For *St*, *Hu*, *Le*, and *Fr*, the two shifts are in opposing directions, one in the direction of contrast and one in the direction of assimilation. For *Ki*, both shifts were in the direction of assimilation, but were too small to be considered significant. When the total number of colors becoming more P after P odors is compared with the total number of colors becoming more P with U odors, it is evident that there has been no tendency in either direction. Tossing of coins could hardly give a more nearly even division than 126 and 129.

Since 5 of the 8 *Os* show no significant shifts in either direction, and the number of colors that shifted in an assimilative direction is practically equal to the number that shifted in a contrast direction, we may conclude at once that alternate presentation of affective stimuli is not, in itself, sufficient to produce either contrast effects or assimilation effects.

The results from the other 3 *Os*, however, show definite shifts. The figures for *Lu* show two very large shifts in the direction of assimilation; the colors became more P when mingled with the P odors, and less P when presented with the U odors again. Both shifts are highly significant by the two statistical methods. For *Sh* and *Ob* both shifts were in the direction of contrast. In each case the two shifts were fairly large, the first shift being slightly larger for both *Os*. The first shifts for both are significant by both methods. The second shift for *Sh* is significant by Method X, while by Method Y the probability that it is significant is 99.9 in 100. The probability

that the second shift for *Ob* is significant is 99.85 in 100 by Method X, 99.76 by Method Y.[9] It seems evident that for these two *O*s we have evidence of definite intermodal affective contrast.

Discussion. In an attempt to discover factors that might have been responsible for the different kinds of results obtained, each *O* was requested to answer a set of questions at the conclusion of the experiment. The questions were as follows:

(1) Did the odors and colors seem to come in one continuous series, in pairs, or as two separate series?
(2) Did you judge the colors and odors on the same scale?
(3) Did you notice any influence of the colors on the odors or of the odors on the colors?
(4) Did you notice if moods of pleasantness or unpleasantness were ever set up by the odors?
(5) Did the pleasantness or unpleasantness of the odors ever last through the colors?
(6) Did you ever make any judgments that were based on memory?
(7) What did you think was the purpose of the experiment?

An examination of the answers to these questions showed no one point with respect to which the two *O*s whose results showed contrast shifts differed from the rest of the *O*s; indeed, they did not agree very well with each other. Analysis of their individual answers, however, seemed to show what had been happening.

Ob had been one of the *O*s in Experiment I, and in that experiment had not given evidence of significant contrast. During that experiment he reported that the odors and colors were entirely separate, "as if there were two experiments at the same time." After the present experiment he reported that there had been a fluctuation between two series and one continuous series. This seems to indicate a gradual coalescence of the two kinds of experiences into a subjectively unitary group, thus producing the condition necessary, according the law of affective equilibrium, for the occurrence of a mass contrast effect. Such subjective grouping between experiences of different modalities, would seem to require long familiarity with the two kinds of stimuli presented in an objectively unitary group. *Sh*, the other *O*, whose results showed shifts in a contrast direction, did not have the background of familiarity with the odors and colors together that helped to produce subjective grouping in the case of *Ob*, and his answers showed that he experienced the odors and colors as forming two separate series. Shortly before starting to observe in this experiment, however, he had read Beebe-Center's paper on affective equilibrium, and had expected some such effect to occur. He was the only *O* who had definitely expected one kind of result while making observations. It is probable that this expectancy was the main factor responsible for the observed contrast effects in his case.

Thus, of the two cases of contrast, one seems to be an instance of intermodal mass contrast of the type described by the law of affective equilibrium, dependent upon the grouping together of the two kinds of experiences. In the other case, the observed contrast may have been an artifact produced by expectancy.

[9]Garrett, *ibid.*, 134.

The answers from *Lu*, the *O* whose results showed marked assimilation, were similar in many respects to the reports of *Tu* in Experiment I. Both reported that moods created by the odors influenced their judgments of the colors. Both also reported two kinds of attitudes. One attitude could be characterized as rather passive and uncritical, with spontaneous judgments; this they correlated with a strong tendency to assimilate. The other attitude was more active, involving more critical judgments; with this, they reported, the tendency to assimilate was weaker, if not entirely lacking. As to the 5 *Os* whose results showed no significant shifts, one of them, *Ki*, had taken part in Experiment I and then had shown significant assimilation. His answers at the close of the present experiment showed that the effect of the U odor set had decreased, in his case, to the point where no definite moods were set up; with the disappearance of the moods, the tendency to assimilate also disappeared. Of the 4 other *Os*, *St* and *Fr* reported no moods and no grouping between the odors and colors. *Le* and *Hu* both reported the presence of moods created by the odors, and also reported definite attempts to prevent these moods from having any effect upon their judgments. *Le* reported no grouping, while *Hu* reported possible grouping between the odors and colors. It is worthy of note here that *Sh*, whose results showed contrast and who reported contrast, also reported that it took effort to avoid judging the colors according to moods induced by the odors. The reports of *Le*, *Hu*, and *Sh* seem to show further instances of the 'critical' attitude reported as occasional by *Tu* and *Lu*.

It would seem, from these results that there are two conditions necessary for the appearance of valid assimilation effects in the results. One is the existence of a fairly strong mood of pleasantness or unpleasantness, created by the odors. The other is the presence of a certain type of attitude which may roughly be described as 'uncritical.'

There is a strong resemblance between the 'critical' and 'uncritical' attitudes noticed in this experiment and the 'objective' and 'physiological' ('intrasubjective') attitudes described by Bullough.[10] But while Bullough has considered these attitudes to be characteristic of different types of subjects, the present writer is inclined to consider them as temporary attitudes which can both be adopted by an *O* at different times in the same experiment.

It is possible that different results would be obtained, if different modalities than those used in the present experiments were employed. It would be interesting, as a check upon the conclusions drawn from the evidence of the present experiments, to see if the tendency to assimilate would become less, if stimuli less extremely P or U than odors are used, and whether the relative frequency of cases of contrast would increase in proportion to the amount of similarity be-

[10]E. Bullough, Recent work in experimental aesthetics, *Brit. J. Psychol.*, 12, 1921, 86.

tween the modalities. An experiment designed to promote subjective grouping between two modalities should demonstrate whether such grouping is necessary for the occurrence of intermodal contrast. A modification of the present technique, requiring the *O*s to make paired comparisons between stimuli of the two modalities, as well as absolute judgments of the individual stimuli, should meet this requirement.

Nothing is known about the relation of affective contrast to somewhat similar phenomena in other fields of judgment. Hollingworth's experiments, which led to his formulation of the concept of the 'central tendency of judgment,'[11] and more recent experiments by Truman and Wever[12] and Wever and Zener[13] have shown the dependence of absolute judgments upon the total experienced situation in other fields. It is possible that a generalization of Beebe-Center's law of affective equilibrium will apply to the method of absolute judgment in any field where such judgments can be made.

Summary

In the two experiments which have been described, an attempt was made to see what changes might be produced in judgments of the affective value of stimuli of one modality, when they were presented intermingled with affective stimuli of another modality. The results may be summarized as follows:

(1) Affective contrast does not usually result when odors and colors are presented alternately in a continuous series. In such a situation, there is no single type of influence by the odors upon the judgments of the affective value of the colors that will appear in every case. In slightly more than half of the cases observed, there was no significant change in the affective value of the colors when P odors were substituted for U odors, or *vice versa*.

(2) Affective contrast, while not the usual result of intermodal affective stimulation, may sometimes occur in such a situation when special conditions are present. It has not been definitely established what these special conditions may be; there is some evidence, however, that subjective grouping by the *O* of the two kinds of experiences into a unitary group is one, and perhaps the main, factor.

[11] H. L. Hollingworth, The inaccuracy of movement, *Arch. Psychol.*, 2, 1909, No. 13, 21-39; The central tendency of judgment, *J. Philos.*, 7, 1910, 461-469.

[12] S. R. Truman and E. G. Wever, The judgment of pitch as a function of the series, *Univ. Calif. Publ. Psychol.*, 3, 1928, 215-233.

[13] E. G. Wever and K. E. Zener, The method of absolute judgment, *Psychol. Rev.*, 35, 1928, 474-477.

(3) When a definitely P or U mood is induced by strongly affective stimuli of one modality, judgments of the affective value of stimuli of another modality, during the same period of time, tend to be influenced in the direction of the prevailing mood. The name *affective assimilation* has been used for such an effect. If the *O* reports himself as adopting a rather critical attitude in judging the stimuli, this tendency will not be apparent in his actual judgments; if he reports himself as adopting a rather uncritical attitude, the tendency will be quite noticeable in the results.

EMOTIONAL EFFECTS PRODUCED BY THE INJECTION OF ADRENALIN

By HADLEY CANTRIL and WILLIAM A. HUNT, Harvard University

In his paper on the emotional action of adrenalin, Marañon[1] stated that the injection of adrenalin produced in his *S*s only a "cold emotion," a feeling "as if afraid," "as if about to cry." True emotions, however, were aroused in a very few cases, both spontaneously and by suggestions; but he attributed these to the emotional predisposition of some *S*s. He therefore concluded that the mere injection of adrenalin, with its consequent stimulation of the sympathetic nervous system, was not sufficient to cause a genuine feeling of emotion. It seemed to the present authors that Marañon did not give sufficient emphasis to those *S*s who reported genuine emotions, and that he was not wholly successful in explaining them by predisposition. Cannon[2] and Wheeler[3] accept his results. Woodworth[4] follows Cannon. Bard[5] even goes to the extreme of not mentioning the cases involving genuine emotion. Landis[6] is more fair in his treatment, and mentions the need for further work on this point. Consequently, the present writers determined to test the emotional results of adrenalin on a group of normal observers, checking carefully any emotions that might appear.[7] It seemed desirable to approach the matter in a purely psychological way, rather than to use the combined clinical, medical, and psychological approach of Marañon.

*Accepted for publication February 14, 1931.

[1]G. Marañon, Contribution à l'étude de l'action émotive de l'adrenaline, *Rev. fr. d'endocrinol.*, 2, 1924, 301.

[2]W. B. Cannon, *Bodily Changes in Pain, Hunger, Fear and Rage*, 1929, 356 f.

[3]E. B. Newman, F. T. Perkins, and R. H. Wheeler, Cannon's theory of emotion, *Psychol. Rev.*, 37, 1930, 305-326.

[4]R. S. Woodworth, *Psychology*, revised ed., 1929, 308.

[5]P. Bard, The neuro-humoral basis of emotional reactions, *Foundations of Experimental Psychology*, 1929, 480 f.

[6]C. Landis, The expressions of emotion, *ibid.*, 511.

[7]The authors were in no way motivated by a desire to prove or disprove the James-Lange theory of the emotions. Their interest lay wholly in the general implications of the work, and not in its application to any specific theory. Indeed, in view of James' exceedingly ambiguous article in 1894 (cf. The physical basis of emotion, *Psychol. Rev.*, 1, 1894, 516), the final formulation of his theory seems so vague and so open to personal interpretation that it appears almost useless for anything but the purpose of controversy.

There was one point in particular on which the work might shed light. This is the supposed relation of pleasantness and unpleasantness to the antagonistic action of the cranio-sacral and sympathetic divisions of the autonomic nervous system. F. H. Allport[8] has based his theory of emotion upon this hypothetical relation. He states: "The cranio-sacral division of the autonomic, supplemented under certain conditions by the cerebrospinal system, innervates those responses whose return afferent impulses are associated with the conscious quality of pleasantness. The sympathetic division produces visceral responses which are represented in consciousness as unpleasantness."[9] In a recent popular presentation, F. H. Lund even goes so far as to call the sympathetic system the "tension nerves" and the cranio-sacral the "pleasure nerves."[10] As Hulsey Cason points out, this relationship still remains to be shown in spite of its widespread acceptance.[11] The feeling states produced by sympathetic innervation through the injection of adrenalin might have an important bearing on the question.

Technique

The general technique involved the intramuscular injection of 1.5 c.c. of adrenalin chloride. The standard Parke-Davis undiluted solution (No. 88) was used. Previous experimentation had shown that a dose of this size would produce a very definite reaction and still be harmless to the patient. As the interest was primarily in the psychological reaction, the only objective check used was the pulse rate. This was taken at frequent intervals. Previous work had shown this to be reliable, as the rate always suffers a slight drop at the onset of the syndrome. A secondary check is the noticeable increase in the strength of the pulse.

The *Os* used were Messrs. E. Land (*L*) and R. Proskauer (*P*), Harvard undergraduates; A. Wheelwright (*W*), graduate student in the Department of Physics; C. BoDine (*B*), Simeon Cantril (*SC*), and J. Goodman (*G*), medical students; S. Rosenzweig (*R*) of the Psychological Clinic; J. Logan (*JL*), a law student, and Mrs. E. Hunt (*EH*). In the first two-thirds of the experiment, the authors also acted as *Os* and their results are included under *C* (Cantril) and *H* (Hunt).

The experiment itself fell into three sections. In the first section, *W*, *L*, *P*, *B*, *G*, *C*, and *H* were all given a single injection of adrenalin. *O* was first asked if he were emotionally predisposed in any way at the time. (If so, he was dismissed until a later appointment). He was then required to rest for 5 min. After this his pulse was taken and the adrenalin injected. The *O* was then seated

[8] F. H. Allport, *Social Psychology*, 1924, 89 f.

[9] *Ibid.*, 90.

[10] F. H. Lund, *Emotions of Men*, 1930, 115.

[11] H. Cason, Pleasant and unpleasant feelings, *Psychol. Rev.*, 37, 1930, 230.

at a table with pencil and paper and asked to write his introspections. His pulse was taken every few minutes. In two cases, two *O*s were seated in the same room, separated only by a screen.

In the second section, *C*, *H*, *L*, and new *O*s, *JL* and *EH*, were each given two injections. One of these injections, unknown to the *O*, consisted merely of a normal salt solution which afforded a check on the validity of the syndrome. The general procedure was the same as above.

In the final section, the experiment was repeated under more rigid conditions to check the previous data. The experimenters themselves did not observe. *JL*, *EH*, *L*, and two new *O*s, *SC* and *R*, were used. Each was given two injections of adrenalin. At the beginning of each sitting the *O*s were asked the three following questions:

Do you at present feel any disposition toward any particular emotional state?
Has anything (either physically or mentally) within the last few days or quite recently happened to disturb your normal state?
Can you recall any dreams that you have had in the past few nights?

If the answers to the first two questions were negative, and the third failed to reveal any disturbing dream content, the experiment proceeded. Otherwise, the *O* was dismissed until a later date. After a 5-min. period of rest, *O*'s pulse was taken and the adrenalin injected. His pulse was then taken again and he was handed the following instructions:

You have been given an injection of adrenalin. You are now asked to report orally to the experimenter and *as fully as possible* any physical or mental symptoms which develop. Do not omit anything, no matter how irrelevant it may seem to you. Try not to become impatient and try to keep your mind free from any material not called up by the actual situation.

The instructions were then removed and *O* was seated in an easy chair before a large grey screen. (In one case, that of *R*, the screen was unavoidably absent.) The *E* sat at a table behind the screen ready to take down the observations. No one else was in the room. *O*'s pulse was taken every 5 min. and also immediately upon the report of the symptoms of the syndrome.

Results

Physical symptoms. The physical symptoms which followed the injection of adrenalin were similar in the three sections of the experiment. The physical syndrome always consisted of a muscular tremor localized particularly in the legs and hands, a tremor in the voice, increased respiration, immediate slight decrease and subsequent rise in the pulse rate, and an increase in the strength of the heart beat. Several *O*s felt a tenseness in the viscera and others reported a tenseness localized in the chest.

Subsequent to the injection of the salt solution in the second part of the experiment, no *O*s reported any noticeable physical effects.

The following are a few typical introspections concerning the physical effects of the injection of adrenalin.

"General muscular tremor and lack of faith in my muscular control. Breathing is more conscious and seems even more necessary than usual." (*C*)

"Definitely conscious of strong heart beat. Feel a tremor in all my muscles, particularly hands and legs. Tremor in voice." (*B*)

"Sudden release of muscular control. A tense feeling in chest and breath feels short. Heart pounding regularly and monotonously—like 'Bolero' (of Ravel)." (*JL*)

It should be mentioned that all the *Os* experienced a prolonged muscular tremor in the arms and legs. This effect lasted in some cases as long as two hours and is worthy of further physiological treatment in connection with the specific action of adrenalin.

Subjective aspects. The subjective aspects will be divided into three parts, corresponding to the three sections of the procedure. (1) In the first part *P*, *W*, and *G* reported only the physical concomitants of the syndrome. The following introspections illustrate this type of report.

"Trembling, particularly in hands. Voice shaky. Legs trembling. No psychic effects." (*W*)

"Trembling definitely in hands and arms. Resulting in difficulty in writing. Tremble in voice. Feel no more pleasant than before and unpleasant only as much as trembling gives me a feeling of unusualness. No emotions at all. Calm and collected. Seem to have all the externals of excitement but no feeling of excitement." (*G*)

B, *C*, and *H* reported a 'cold' emotion.

"Feel as though I had had a big scare, not fright but like a reaction after tremendous fright. Don't feel unpleasant. If there is any affective state it is somewhat pleasant. Certain bodily restlessness but no worries, cares, or mental anxieties." (*B*)

"Feel as I do either before an event in which I am to take an active part—such as a public appearance or athletic competition or before some great joy which I am anticipating. Likewise, it is much the same as the feeling I have after being scared for an instant by a danger which passes almost as soon as it appears. General restlessness and lack of faith in my muscular response—and yet I want to do something to relieve this uneasiness. . . . An undirected uneasiness without cause and purpose and hence I am annoyed. No positive pleasant or unpleasant emotions but the desire to have one of some kind to relieve my restlessness." (*C*)

"Bodily feeling of extreme nervousness. The bodily feeling almost swamps the psychic state, but the nervousness is wholly a matter of physical symptoms. I feel nervous, but I am not nervous. The reason seems lacking. I lack an object and the intellectual state is not fulfilled. To sum it up, I feel vaguely nervous, excited, but any definite emotion is lacking, and the excitement is a detached, bodily affair." (*H*)

L seemed to get something resembling a genuine emotion.

"Excitement (have felt like this in large class when I think I have a good idea, when raising my hand). No elation, but a background of elation. Feel light, as I used to in a burst of creative activity (writing). A pleasant state, possibly by association with such activity. Not unfamiliar. Used to attempt to bring on such states deliberately in adolescence. . . . During times of tremendous intellectual experiences in the past, times when my vision seemed peculiarly clear and far reaching, I have experienced similar reactions, but not even then so strongly. A tremendous illusion of power, vitality." (*L*)

(2) In the second section of the experiment no *O*s reported any subjective symptoms subsequent to the injection of the salt solution.

Following the injection of adrenalin, *JL* noted marked physical changes but no psychic accompaniment.

"Muscles relaxed—sensation of no control. Throbbing of pulse through chest and back of head. Physical sensation as violent as any emotional reaction but no emotions present. Rather a distinct lack of them." (*JL*)

A 'cold' emotion was reported by *L* and *C*.

"Whole reaction seems localized in chest. Feel like breathing deeply to shake it off. Heavier and more and more like a feeling of great apprehension, except entirely localized. More accurately, chest feels as it does during great apprehension, but this feeling so localized that I can introspect detachedly as if I were considering a slightly injured finger. This reaction was dominated by a lack of confidence, a desire to withdraw, in short all the elements of fear except mental pain." (*L*)

"Feeling which is neither pleasant nor unpleasant. General excitement and I should like an object for it. Some annoyance at being unable to find relief from the excitement by some action." (*C*)

In the introspections of *H* and *EH* there is a genuine emotion noticed.

"The sound of my own voice trembling acts as context for fear. Feel extremely worried. Not exactly anxiety but a foreboding which is unpleasant. Whenever, without my mind on the symptoms, I do something like putting my hand to my face, the trembling of the hand gives me—on its perception—an extreme anxiety—not fear—but foreboding. No definite object but nevertheless an emotion." (*H*)

"I seem oppressed with a vague fear of something—feeling much the same as when I've lain awake all night, frightened that Bill might die. In spite of knowing the cause of his illness the fear was not specific and neither is this. I am oppressed with a nameless fear. I want to get away from it just as the night of Bill's illness I wanted to relax and sleep and tried to rationalize my fears. But that was impossible then and it is impossible now. It is decidedly unpleasant." (*EH*)

(3) In the third part the mere physical symptoms of the syndrome were reported once by *EH* and *SC* and twice by *R*.

Reports of 'cold' emotion were given once by *SC*, *L*, and *EH*, and twice by *JL*.

"A feeling in my abdomen as if I were going to get into an argument. Feeling as when somebody tells me they don't believe in vaccination." (*SC*)

"Felt a bit pleasant a moment ago. This succeeded by a disturbed feeling now, neither pleasant nor unpleasant. Decided predisposition to an emotion. Only a constant conscious attempt to keep calm prevented an emotion. It was an effort to keep calm." (*L*)

"Tenseness. The feeling as though something were going to happen that I expected to be disagreeable. An overwhelming desire to have something happen through which I might relieve the suspense." (*EH*)

"Feeling of something exciting going to happen. A desire for action. Throughout reaction there is a definite feeling of unrest which cannot be described as

pleasant although there is nothing clearly unpleasant—might be summed up as uneasiness. I feel that during the whole reaction I should be readily subject to any kind of emotional suggestion." (*JL*)

"I feel very carefree and pleasant. Pleasant state of being continues although heart beat and trembling increased. Now a feeling of uneasiness and indifference. Feel as if I could either laugh or cry. As you do when drinks are being mixed and you are afraid you won't get one but hope you will. I want to do something but don't know what it is." (*JL*)

L reported a genuine and sudden emotion.

"Extreme fear was present, but no content for it at the time. Probably an unconscious reason but at the time nothing but fear. The strongest reaction yet. I found myself shaking, chest trembling, building up rapidly in intensity, whereupon I abruptly recognized that I was intensely afraid." (*L*)

It will be seen that while the majority of reports show no emotion, or at best a 'cold' one, there are cases of genuine emotions aroused which seem difficult to explain away as due to predisposition. The important thing to be noticed is that the bodily symptoms may occasionally satisfy some of the *O*s, while at other times they demand an intellectual content or reason before they will report an emotion. Some of the *O*s never appear able to experience an emotion without this content. In those *O*s not experiencing an emotion, there frequently appears a need for catharsis, a need to throw off the tension by completing the reaction. It is a desire to get the matter 'off their chests' by having an emotion and releasing the tension aroused. It is also apparent that there are no strict affective correlates of the adrenalin syndrome. The *O* may find it pleasant, unpleasant, or affectively indifferent. In one case we even have an exceedingly pleasant state resembling elation.

These results are summed up in the following Table I.

TABLE I

SUMMARY OF THE RESULTS OF THE EXPERIMENT

Classification of Reports	No. Reports
Total injections	22
Physical symptoms	22
Pleasant emotions	1
Unpleasant emotions	3
'Cold' emotions	10
No emotions	8
Unpleasant affect	3
Pleasant affect	3
Indifferent	5
No mention of affect	11
Need for catharsis	6

Conclusions

On the basis of the above results we may conclude that in general the injection of adrenalin is not sufficient to produce an emotion. A 'cold' emotion may be aroused, but the lack of any object or reason for the emotion usually deprives it of its genuineness. In a few cases, however, the injection of adrenalin with its subsequent bodily symptoms is adequate to produce a genuine emotion; the mere bodily concomitants are sufficient for an emotional state without the presence of any object or reason.[12]

The experimental findings of the present investigation agree in general with those of Marañon but the present authors differ from him in their interpretation of the genuine emotions aroused in their experiment.

While the results of the present experiment may not justify the formulation of a general theory of emotions, the writers wish to venture a few theoretical implications. It seems to them that an analysis of the conscious emotional state would reveal its dependence upon the following possible factors.

(*1*) *A primary autonomic reaction* centered in the autonomic nervous system and involving changes in the vasomotor system, the respiratory system, the viscera, and certain peripheral changes such as sweating and muscular tremor.

(*2*) *Secondary behavioral reaction patterns*, both instinctive and learned, dependent upon the cerebro-spinal nervous system. This includes such reactions as flight, attack, facial expression, and vocalization.

(*3*) *An intellectual content.* By this we mean the presence in consciousness of some object or situation to which the emotion is directed and around which the emotion is intellectually organized. This involves the active participation of the higher logical processes, probably centered in the cerebral cortex.

The *usual* emotional state seems to involve a combination of (*1*) and (*3*). However, as the only criterion we have of a conscious emotional state is the *O*'s acceptance of it as such, the writers believe, on the basis of their results, that (*1*) is the only *sine qua non* for an emotion, for it may in some cases suffice itself to produce the

[12]These emotions resemble that described by Carl Sandburg as follows:

"We are afraid. What are we afraid of?
We are afraid of what we are afraid of.
We are afraid of this, that, these, those, them."

emotional state. Nevertheless, such cases are rare. The status of (*2*) is at present unsettled, but this factor is probably of secondary importance.

The tendency of Marañon and others to explain the genuine emotions arising from the adrenalin syndrome by referring to predisposition seems unjustified to the present authors. True, in some cases, in addition to the report of the emotion, the *O* reported an associative content, viz. some reference to previous experiences or memories. But like associative content was also present in the report of 'cold' emotions, so that the difference cannot be explained on this basis. It seems to the authors that the differences of report are due to some critical attitude assumed by the *O*. In other words the situation must contain certain logical relationships in order that most *O*s experience a genuine emotion. In some cases, however, such a relationship is unnecessary. On the physiological level this may be due to differences in the strength of cerebral inhibition. These differences may be present not only between *O*s but within the same *O* at different times.

No doubt the physiological reaction involved in an emotion includes the basis for an affective state, but the report of pleasantness and unpleasantness seems to depend upon a special affective judgment which is not necessarily included in the report of the simple emotional consciousness.

The lack of any constant affective correlates of the adrenalin syndrome indicates that the theories which base unpleasantness upon the activation of the sympathetic division of the autonomic nervous system are palpably unfounded.

Finally, the need for cartharsis reported by some *O*s would seem to show that the emotional reaction contains within itself a definite drive to completion through expression. This resembles the tendency to closure exhibited by *Gestalten*.

LOCALIZATION OF BRIGHT AND DULL PRESSURE*

By WILLIAM A. HUNT, Harvard University

In some unpublished work mentioned by Hoisington,[1] Horiguchi reports the localization of the pressures established by Nafe in his study of the affective processes.[2] Bright pressure is localized in the general region of the shoulders and neck, and dull pressure in the abdominal area. In two previous investigations of bright and dull pressure, the present author found his *O*s substantiating this localization.[3] Its manifest importance made him desire a more careful investigation under conditions as well controlled as possible. As the localizations reported by his *O*s had referred to general bodily areas and had varied with different *O*s, he decided to concentrate on what seemed to be the most important aspect of the matter, namely, that bright pressure was always localized *above* dull pressure.

In order to minimize the influence of any bias on the part of the *O*s, it seemed best to use the method of correlation. Thus the *O*s were first to report on the localization of the bright or dull pressures aroused by a series of stimuli which could then be assigned values signifying the bodily locus of the pressures involved. At a later sitting affective values were to be ascertained for the same series of stimuli. Correlation between the two series would then reveal whether or not pleasant stimuli arouse pressures in a bodily locus in constant relation to that of the pressures aroused by unpleasant stimuli. The correlation of bright and dull pressure with pleasantness and unpleasantness, which is assumed by this procedure, is justified by the author's previous work.[4]

At the same time it seemed promising to investigate the presence of any other internal bodily pressures. For this purpose a series

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[1]L. B. Hoisington, Pleasantness and unpleasantness as modes of bodily experience, *Wittenberg Symposium on the Feelings and Emotions*, 1928, 237-240.

[2]J. P. Nafe, An experimental study of the affective qualities, this JOURNAL, 35, 1924, 507-544.

[3]W. A. Hunt, The relation of bright and dull pressure to affectivity, this JOURNAL, 43, 1931, 87-92; The pressure correlate of emotion, *ibid.*, 43, 1931, 600-605.

[4]*Opp. citt.*

was inserted in which the *O*s were merely asked to localize any bodily pressures present, no mention being made of either bright pressure or dull pressure.[5]

PROCEDURE

Twenty colored papers of varied hue and brightness were used as stimuli. They were exposed through an aperture, 5 in. square, in a large screen, 6 ft. high by 4 ft. wide, with a 2-ft. wing on each side. The screen was painted a cream gray. Its size enabled *E* to sit at a table behind it, unseen by *O*. The large gray field aided *O* in concentration by shutting out distracting stimuli. *O* sat in a big easy chair, which permitted extreme relaxation and removed many of the confusing bodily tensions accompanying normal posture. His eyes were about 4 ft. away from, and on a level with, the aperture in the screen. The position of both chair and screen was kept constant. Lighting was artificial and constant.

In order to prevent the bright and dull pressure judgments from influencing the judgments of other internal bodily pressures, the latter judgments were taken first. The *O* was told to sit in a comfortable, relaxed position, and was then given the following instruction:

I shall present various colored stimuli. You are to attend to the general experience involved in observing them, and to notice whether or not this experience includes any internal pressures. If such internal pressures are present, please localize them on the chart and report the number to the experimenter.

The instructions were then removed and *O* was given a chart consisting of an outline of the human torso. A line running down through the middle of the body was divided into equal sections by the numbers 1 to 8. 1 was at the top in the region of the upper neck and head, and 8 at the bottom in the region of the lower abdomen. The series of colors was then presented three times, in constant order, with about a 10 sec. interval between successive colors. There was a brief rest at the end of each series. The localizations were recorded and *O* was dismissed for two weeks.

After this time had elapsed, he was called in again. This time the instructions were:

I shall present various colored stimuli, and you are to judge whether they are pleasant, unpleasant, or indifferent.

The same colors and procedure were used, and the affective judgments were recorded.

Two weeks later at a final session, *O* was given these instructions:

I shall present various colored stimuli. You are to attend to the general experience involved in observing them, and to notice whether or not this experience includes any bright or dull pressures. If such bright or dull pressures are present, please localize them on the chart and report the number to the experimenter.

'Bright pressure' resembles tactual bright pressure, or a touch quality lying between bright pressure and tickle. It is a diffuse bodily feeling with expansive qualities. In addition to its decided brightness, it may be described as buoyant, lively, elusive, and welling. It is like expansiveness of the body.

'Dull pressure' has been described as dull pressure or a quality lying near dull pressure, or between dull pressure and strain. It is not extended and has qualities of constriction. It is dull, inert, heavy, sinking, leaden, condensed, and like bodily contraction.

[5]The writer wishes to express his indebtedness to Dr. J. G. Beebe-Center of the Harvard Psychological Laboratory for his constant assistance and encouragement.

Thus the experiment consisted of three series dispersed over a period of six weeks. A series of localizations of internal pressures was followed by a series of affective judgments, and this by a series in which *bright* and *dull* pressures were localized. Seven *O*s were used. Four of these, Mr. H. Cantril (*C*), Mr. E. Land (*L*), Mrs. E. Hunt (*H*) and Mrs. H. Taylor (*T*), had previously observed in the writer's experiments on bright and dull pressures. The remaining three, Mr. J. Volkmann (*V*), Dr. O. Trimble (*Tr*), and Mr. H. Odbert (*Od*), were new to the problem. Originally, one other naïve *O* had been included, but it was necessary to drop him due to his failure to keep the experimental appointments.

The treatment of results was the same for both pressure series. The localizations had been reported through numbers corresponding to different sections of the torso as marked on the chart. These numerical values were then averaged for each stimulus, and the result was taken to indicate the average locus of the pressures aroused by that stimulus. Thus the values for each series fell into a distribution representing the dispersion of these average localizations over the torso, from top to bottom. Where no localization was reported, the judgment was dropped out and not allowed to affect the average for that stimulus. In the few cases where no localizations were reported for any of the three presentations of a stimulus in a series, these stimuli were merely discarded from the distribution, and in computing correlations the corresponding number of the other series was discarded also.

The affective judgments were also assigned numerical symbols: a value of $+1$ was given a judgment of pleasant, 0 to one of indifferent, and -1 to one of unpleasant, and the sum of these values for each color was considered to represent the degree to which that color elicited pleasantness rather than unpleasantness. The series was then arranged in a distribution with the positive, pleasant values at the top. Correlation of this series with each of the pressure series would then give a coefficient representing the degree to which pleasantness was accompanied by pressures *above* those accompanying unpleasantness.

Results

In the first series, that concerning *any* internal bodily pressures, all the *O*s reported localized pressures except *Od*. He gave occasional judgments but claimed they were extremely difficult to make and hence were exceedingly doubtful. The 4 experienced *O*s reported that the pressures observed were not bright and dull pressure as such, but were ordinary localizable pressure sensations in the body cavity. Correlation of this series with the affective series gave the following results:

	Observer					
	H	*C*	*T*	*L*	*Tr*	*V*
r	.90	.91	.75	.89	.85	.66
P.E.	.04	.03	.07	.03	.04	.08

In the bright and dull pressure series, all the *Os* except *Od* once more reported localization of the pressures. The correlations with the affective series were:

	Observer					
	H	*C*	*T*	*L*	*Tr*	*V*
r	.93	.94	.70	.97	.85	.83
P.E.	.03	.02	.08	.01	.04	.05

It will be noted that there is a slight tendency, of doubtful statistical significance, for these values of *r* to be higher than those of the first group.

Correlation of the two pressure series with each other yielded the following coefficients:

	Observer					
	H	*C*	*T*	*L*	*Tr*	*V*
r	.90	.91	.93	.87	.83	.73
P.E.	.04	.03	.02	.04	.05	.07

Od was given another trial in order to see if further practice would overcome his difficulty. It did, but, because of the doubtful influence of training, his results are not considered with the main group. In the second trial his judgments came with greater ease and gave *r* of 0.86, 0.89, and 0.93, which are in keeping with the other results.

These results concern only the relative localization of the pressures accompanying hedonic tone. The data, however, also yield information of the absolute localization of these pressures. Table I gives the frequency distribution of the localizations according to *O*, series, place of localization, and the supposed affective

TABLE I

FREQUENCY DISTRIBUTION OF PRESSURE LOCALIZATIONS

Capital letter indicates *O*. P = pleasant stimuli. U = unpleasant stimuli. a = simple pressure localizations. b = bright and dull pressure localizations.

Number of Cases

BODILY LOCUS		*H*				*C*				*I*				*L*				*Tr*				*V*			
		P		U		P		U		P		U		P		U		P		U		P		U	
		a	b	a	b	a	b	a	b	a	b	a	b	a	b	a	b	a	b	a	b	a	b	a	b
NECK	1									2	3	1						1	4						
	2										4		3					5	4	2	2	12	2	4	
	3	1	9			11	19			5	3	1	3	4	6	1		3	4	5	7		19		
CHEST	4	17	9		1	8	7	1			1	5	6	2	2	5	6	4	3	4	1	7	9	2	2
	5				1	2		6	2		1	1	3	2		22	21	3					2	2	3
	6					4		11	10			2	4				1		2	4	2	7		5	4
ABDOMEN	7			7	2			9	9	2		11	5						1	6	9		1		4
	8				4				6									1		10	12				

value of the stimulus. It should be remembered, however, that in an endeavor to rule out any bias, the *Os* were asked in the pressure series only to report the number corresponding to the bodily locus. No mention was made of the affective

value of the stimuli. This last was determined in the special affective series coming midway between the two pressure series. Since the affective values of the stimuli were established in a separate sitting, it is only right to mention the fact that affective variability working between the series may have caused some error in the table. In an endeavor to control this variability the frequency distributions contain only the localizations of those stimuli whose affective value was constant or included one judgment of indifferent. Stimuli reported as both pleasant and unpleasant in the affective series were discarded, as were those whose reported value contained two or more judgments of indifference. It was felt that to include these data would allow too great an error due to variability.

The frequency distribution, while showing clearly the tendency of pleasantness to produce pressures localized above those produced by unpleasantness, nevertheless shows considerable dispersion within each category and some overlapping between the categories. The failure of the *Os* to report a constant particular locus for the pressures accompanying either affective tone may be due to the known difficulty of localizing organic sensations. None of the *Os* found the task easy. On the other hand, due to the fact that a few *Os* occasionally reported that pressures with a dominant locus in one zone would extend over into neighboring zones, it may be that we are dealing with a general reaction whose specific elements may vary at different times. The constancy of the above-below relationship between the pressures seems to demand some such unifying factor. Even where the dispersion within each category is great, the relative position of the pressure components of one category to those of the other remains well marked.

All the *Os* reported that the two different pressure series were made under distinctly different attitudes of introspection and that the simple pressures were different from bright and dull pressure. Bright-and-dull pressure seemed to be a larger, more inclusive category, with the stress less on the actual specific pressure components and more on the general reaction involved. Four *Os* spontaneously suggested that while bright-and-dull pressure seemed to be different from the simple bodily pressures reported, these last might well be specific components of a general complex of sensations corresponding to bright and dull pressure.

Conclusions

While our results tend to confirm Horiguchi's localization of bright pressure in the general region of the shoulders and neck, and dull pressure in the abdominal area, they stress the importance of the central areas of the chest for bright pressure and the occurrence of overlapping in the lower chest regions. The localizations are within *general* areas, and specific localizations show marked variations, both in the case of single individuals and within the group. On the other hand, our data show that the relationship between bright pressures as a whole and dull pressures as a whole is remarkably constant, and we may definitely conclude that bright pressure is localized *above* dull pressure. We may also conclude that pleasantness

and unpleasantness are accompanied by simple bodily pressures which maintain the same relative positions as bright and dull pressure.

It seems too early to attempt any definite explanation of these results. A suggestion, however, of the direction in which such an explanation may lie is afforded by the following passage in an article by Zoneff and Meumann. "Es sei noch bemerkt, dass auch die gesammte Haltung des Brustkorbes bei Lustgefühlen eine andere ist als bei Unlust. In Zuständen, die als 'Lust' bezeichnet werden, hebt die Versuchsperson merklich den Brustkorb, was sich in dem Steigen des Niveaus der gesammten Curve auf der Kymographiontrommel andeutet (umgekehrt bei Unlust). Es ist die Erscheinung, die man in dichterischer Sprache als 'geschwellte Brust' bezeichnet."[6]

[6] P. Zoneff and E. Meumann, Über Begleiterscheinungen psychischer Vorgänge in Athem und Puls, *Philos. Stud.*, 18, 1901, 60.

AN EXPERIMENTAL STUDY OF 'SEARCH'

By Howard L. Kingsley, Boston University

In a former investigation a comparative study was made of perception, thinking, and that scrutinizing, exploratory type of performance called 'search.'[1] The problem was to discover the natural relations which these functions sustain to each other. The search-problem there called for the exploration of a picture to discover some specified detail. In the present study the purpose has been to secure a more complete description of this type of function by making further observations upon the processes of exploration and discovery under conditions similar to those of the previous experiment.

Method and procedure. A series of 12 pictures, each mounted on cardboard 9 x 12 in., was presented to the *Os* by means of an exposure apparatus under the following instructions:

"At the 'ready' signal you are to fixate the center of the screen. At the 'now' signal a picture will be presented and at the same time you will be told to find some object in the picture. When you have located this object press the button and immediately report your total experience of the exposure period."

The object named by *E* was always an integral part of the picture but one not likely to be noticed in a casual glance. The *Os* were *B*, *R*, *Br*, *M*, and *T*. *B* and *R* were members of the psychology staff of Goucher College and the others were students majoring in psychology. Sixty observations were made.[2]

Results

Immediately following the withdrawal of the shutter there was a hurried survey of the picture which gave *O* some notion of the locale of the search. Then came *E*'s prescribing statement, *e.g.* "Find the tablespoon," "Find the rabbit," the comprehension of which gave definite form to the search-task and thus determined the course and objective of the performance.

(*1*) *Visual exploration.* The task set up by the conditions impelled *O* to explore the picture visually until the designated object

*Accepted for publication December 15, 1930.

[1] H. L. Kingsley, Search: A function intermediate between perception and thinking, *Psychol. Monog.*, 35, 1926, (no. 163), 16-55.

[2] The writer acknowledges his indebtedness to Dr. Ethel Bowman and to Misses Elizabeth Rutherford, Theresa Berney, Charlotte Towson, and Anna Ruth Meeks. The experiments were done in the psychological laboratory of Goucher College.

(hereafter called X) was discovered. The search was characterized by "shifting of the eyes," "scanning of various objects" in the picture, numerous "shifts of fixation" and "glancing about" from one part of the picture to another.

Thus while engaged in this exploratory quest all *O*s experienced a succession, usually rapid, of visual sensory patterns. The number and character of these experiences varied with the *O*s and with the nature of the picture. For example, *B*'s search took the form of an excursion around the picture noting one object after another and rejecting each in turn until X was apprehended. *R*, as a rule, did not check off the various objects in this manner. Discrete perceptions were less numerous in her experience and a correspondingly larger place was filled by visual imagery. Some pictures, moreover, presented a greater wealth of attention-commanding details than others. Central and conspicuous figures, brightly colored objects and novel features naturally touched off perceptions.

Fixation at first was usually on the center of the picture. From the center the eye-movements were most frequently to the upper left corner and from there across to the right side, then down to the bottom. Further than this the course of the fixations was irregular and unordered except where *O* made use of clues considering where the object was likely to be found. "Exploration was haphazard until the ship (X) was found" (*Br*). "The discovery was due to chance movement in the right direction" (*R*).

The search-task which initiated the exploratory performance was sustained by pressures and strains or verbal kinaesthesis. "Pressure in my head made me want to go on. There was a lack of satisfaction in not having found X, carried by these pressures, which urged me on" (*T*). "Verbal kinaesthesis, 'Find the bread-knife, find the bread-knife' " (*Br*). "Repeated the question to myself" (*M*).

The meaning that search was in progress was carried by strain qualities arising from muscular tension throughout the upper part of the body, especially the kinaesthesis of eye-movements and pressures due to "narrowing the eyes." "There was not much feeling of search this time. The feel of muscular contraction in the head and neck was not so pronounced as in the more difficult cases of search. In more difficult search the muscular tension is greater" (*Br*).

Visual images of X frequently appeared during the search and sometimes they materially influenced its course and outcome; though their appearance was less frequent for some *O*s than for others.

The performance was enlivened by varied affective tones and colored by unanalyzed attitudes of confidence or uneasiness as to the outcome.

Elaborative thinking sometimes obviated piecemeal examination of the picture and frequently abbreviated the process. The *O*s made use of suggestions derived from former experiences and clues discovered in the pictures. "I reasoned. I did not just let my eyes wander around. I looked for the table or furniture on which the ship model [X] would be located and so avoided aimless eye-wandering" (*M*).

(*2*) *Discovery and identification of the object.* Sometimes *O*'s idea of X prior to discovery did not coincide with the object as pictured. This lack of agreement frequently, though not always, hindered discovery and identification. *R* had difficulty in locating a bread-knife that was pictured as thrust through a loaf of bread so that only the handle and a small part of the blade were visible because she looked for a bread-knife whose entire blade was showing. Sometimes, in spite of such disparity, *O* realized at once, upon seeing X, that it was what she was to find. This usually was the case where *O* had experienced the object under various conditions or in different forms. But there were times when *O*, upon seeing X, decided that that, rather than what she had imagined, was what *E* meant. When asked to find "the miniature sailing vessel," *B* understood *E* to say, "miniature of a sailing vessel," and that meant for her a small picture of a sailing vessel. In her report she said, "Then a group of grays stood out. Verbal kinaesthesis 'Sailing vessel'; oh yes, miniature sailing vessel, not miniature of a sailing vessel."

When the perceived object did not match *O*'s idea of X and such reconciliation was not made, the object was rejected and the search continued. *M* reported that when looking for a turkey she tried to make chickens look like a turkey but this attempt proved futile so her eyes moved on.

The discovery of X was commonly announced by some such verbal response as "there," or "that's it." "Then, in fragmentary verbal kinaesthesis, 'Yes, that must be it.' Then the key was pressed" (*B*). The qualities of the concluding perceptual experience were closely integrated and highly focal. "Then I saw the bird. It stood out in the foreground. Everything else receded to the background" (*R*). When X was clearly and definitely identified there usually came a feeling of satisfaction and a relaxation from the tension

of the search. "There was a pleasant feeling, a realization of something accomplished and that I had reached my goal" (*T*).

(*3*) *On overlooking familiar objects.* Titchener found the motor set and the visual image to be the essential factors in the discovery of a familiar object for which one is looking.[3] It appears that while there was the discharge of a verbal-motor set in our observations the discovery of the object depended not upon the type of motor set but upon the perceptual set. When the *O*s gazed at X and failed to apprehend it as the object they were to find, it was because of inadequate preparation for such apprehension. While such inadequate preparation occurred in cases where there was no visual image of X it often appeared in the form of an incorrect expectation carried by a visual image that did not 'fit.'

In his note on overlooking familar objects Ruckmick states that failure to see a familiar object before him and for which he is looking is due usually to the "unsuitable image." He adds, "Now the chief feature about the mental image that makes it inadequate seems to be its size—it is too small."[4] In our experiments the visual image differed from the sight of the object in a variety of ways. It changed during the search and differed from X in size, color, and form. But the fact that the image was not a replica of the object did not always cause the latter to be overlooked or delay its identification. When looking for a turkey where the head only appeared in the picture, *R* had a visual image of an entire turkey and was surprised to find only the head. There was no hesitation, however, in recognizing the turkey-head as the thing she was to find. Where *O* had experienced the object in many situations it was recognized readily even if the visual image did not 'fit.' In some accounts the image was described as 'generalized.' It seemed to mean the object regardless of particular mode, shape, size, or position. The important thing was the meaning conveyed by the image. If *O* expected something different from the X-picture she was likely to encounter difficulty in bringing her search to an early and successful conclusion.

Conclusions

The search-performances studied in these experiments were characterized by exploratory eye-movements, rapid shifts of attention, and corresponding changes in visual patterns. The task was sustained mainly by verbal kinaesthesis, and pressures and strains in the head. The search-meaning was carried by strains from the muscles of the eyes, head, and neck. Visual images of X contributed to the comprehension of the question and the anticipation of X. Where the meaning carried by the image agreed with the meaning of the perception, the observer identified X readily even when the image did not resemble the perceptual pattern. Disagreement between *O*'s idea

[3] E. B. Titchener, The overlooking of familiar objects, this Journal, 35, 1924, 304-305.
[4] C. A. Ruckmick, On overlooking familiar objects, *ibid.*, 37, 1926, 631.

of X and the picture of X did not hinder recognition when *O*'s experience with the object was extensive. The search was sometimes unsuccessful because *O*'s idea of X was inadequate. The search-performace was rich in affective tones and mental postures. It was facilitated by the use of suggestions and clues. Its termination came with the discovery of X in the form of a highly integrated and focalized perception, usually announced by a response in verbal kinaesthetic terms and accompanied by relaxation and satisfaction.

SOME SEX DIFFERENCES IN FACTUAL MEMORY

By Alfred G. Dietze, University of Pittsburgh

Sex differences in memory have been studied by numerous workers with conflicting results. The findings of many lead them to assert the superiority of girls and women at all ages and with all degrees of training. Among these are to be mentioned especially Burt and Moore, Gates, Pyle, Schuyten, Shaw, and Whipple.[1] In the work of Lobsein, Lyon, Mulhall, Netschajeff, and Schramm the results cannot be interpreted as definitely in favor of the feminine sex.[2] Finally, Aall and Vos, both of whom tested for logical memory, report male superiority.[3] The question is difficult to settle decisively because many factors complicate the selection of groups and cast doubt upon their comparability. In view of the conflicting findings and of the difficulties in the way of investigating the subject, there is no justification for a dogmatic stand. The present writer offers

*Accepted for publication February 7, 1931.

[1]G. Burt and R. C. Moore, The mental differences between the sexes, *J. Exper. Ped.*, 8, 1912, 273-284, 355-388.

A. I. Gates, Experiments on the relative efficiency of men and women in memory and reasoning, *Psychol. Rev.*, 24, 1917, 139-146.

W. K. Pyle, *The Examination of School Children*, 1913, 8-14; *The Psychology of Learning*, revised ed., 1928, 147-151.

M. C. Schuyten, Sur les methodes de mensuration de la fatigue des écoliers, *Arch. d. Psychol.*, 4, 1904, 113-128.

J. C. Shaw, A test of memory in school children, *Ped. Sem.*, 4, 1896, 61-78.

G. M. Whipple, *Manual of Mental and Physical Tests*, 1921, 211-213. This author also summarizes the results of other investigators, cf. 179-184, 213 f.

[2]A. H. Chamberlain, A memory test with school children, *Psychol. Rev.*, 22, 1915, 71-76.

M. Lobsein, Experimentelle Untersuchungen über die Gedächtnisentwickelung bei Schulkindern, *Zsch. f. Psychol.*, 27, 1901, 34-76.

D. O. Lyon, The relation of quickness of learning to retentiveness, *Arch. Psychol.*, 1916 (No. 34), 1-60.

E. F. Mulhall, Tests of memories of school children, *J. Educ. Psychol.*, 8, 1917, 294-302.

A. Netschajeff, Experimentelle Untersuchungen über die Gedächtnisentwickelung bei Schulkindern, *Zsch. f. Psychol.*, 24, 1900, 321-351.

F. Schramm, Zur Aussagetreue der Geschlechter, *Zsch. f. ange. Psychol.*, 5, 1911, 355-357.

[3]A. Aall, Zur Psychologie der Wiederzählung, *Zsch. f. ange. Psychol.*, 7, 1913, 185-210.

H. B. L. Vos, Beiträge zur Psychologie der Aussage, *Zsch. f. ange. Psychol.*, 4, 1911, 375-378.

some results which seem to support the third of the above positions in indicating a superiority of boys over girls in tests of factual memory given to children of grades VII to XII inclusive.

Method. Pyle states that experimenters reporting male superiority in memory tests make their comparison by grade and not by age. Since in the same grade the boys tend to average higher in age than girls, this is not an adequate basis for comparison. An adequate sex comparison should be made by age, not by grade.[4] In keeping with Pyle's suggestion, the present writer sought to avoid both difficulties by pairing the *Ss* into parallel sex groups by both age and grade. The *Ss* were chosen from a much larger number of another experimental group in such a manner that every boy had opposed to him in the other group a girl in the same grade and of the same age within two months. Three sets of such paired groups were used.

The *Ss* were given printed pamphlets containing a short article approximately 1000 words in length and were instructed to read it carefully a single time. When they finished reading they were given a comprehensive test covering the facts contained in the article and were requested to answer the questions as carefully as possible. A different article was used for each set of paired groups, and the tests covering them contained from 100 to 125 multiple-response type questions. The articles are entitled *Radium: The Magic Metal, The Early Germans,* and *Sir Richard Arkwright,* and will herein be referred to by their key words. The articles are of suitable difficulty for pupils in the junior and senior high schools, and the tests possess a highly satisfactory degree of objectivity, reliability, and validity.[5]

Results. The means for the respective groups, the means of all the *Ss* of a given sex reading an article and their standard deviations, and the data on the differences of the means are given in Table I. The scores are in terms of percentage of items answered correctly.

A study of the table reveals that, in the case of *Radium,* 8 of the 9 comparisons of age groups favor the boys and 1 the girls; in the case of *Germans,* 6 out of the 9 comparisons favor the boys and 3 the girls; and in the case of *Arkwright,* 6 out of 8 comparisons favor the boys, 1 the girls, and 1 ties. Thus in 20 out of 26 comparisons the boys have the higher average scores, in 5 the girls are superior, and in 1 no advantage lies with either sex.

Comparing the averages for the total groups, we find the mean of the boys to exceed that of the girls in *Radium* by 3.7. Since the standard deviations of the two groups are 12.0 and 11.9 respectively, the probable error of this difference is .74, and the difference is 5.04 times its probable error and completely reliable. Similarly the differ-

[4] *Psychology of Learning,* 1928, 150.

[5] Cf. A. G. Dietze, Factual memory of secondary school pupils for a short article which they read a single time, *Univ. of Pittsburgh Bull., Graduate School Abstracts,* 6, 1930, 39-46.

TABLE I

AVERAGE IMMEDIATE FACTUAL MEMORY OF PARALLEL SEX GROUPS PAIRED BY AGE AND GRADE

Age	Radium (243 *Ss*)			Germans (220 *Ss*)			Arkwright (198 *Ss*)		
	No.	Boys	Girls	No.	Boys	Girls	No.	Boys	Girls
11	6	64.2	75.0	7	63.9	49.6	6	50.0	45.8
12	40	72.4	70.1	31	59.1	60.7	14	54.6	50.4
13	35	70.4	68.1	36	59.0	54.4	19	54.3	50.1
14	42	75.6	69.0	51	61.8	63.0	26	50.9	50.2
15	46	78.0	73.0	28	62.5	62.2	52	53.8	49.3
16	32	80.0	75.2	37	70.7	71.3	23	62.3	54.0
17	30	81.2	77.3	14	80.0	73.2	38	62.8	63.2
18	11	80.2	78.9	11	74.8	68.4	20	62.5	62.5
19	1	82.5	57.5	5	77.5	70.5			
Av.		75.8	72.1		64.9	63.3		57.0	54.3
S.D.		12.0	11.9		15.4	16.6		13.3	13.3
Diff.		3.70			1.60			2.70	
P.E.$_{Diff.}$		0.74			1.03			0.90	
Diff./P.E.$_{Diff.}$		5.04			1.55			2.99	

ence of 1.6 in favor of the boys in *Germans* is 1.55 times its probable error, or little better than chance; and the difference in *Arkwright* is 2.99 times its probable error and, therefore, fairly reliable.

Conclusions. We need not necessarily interpret the above results as contradictory to Pyle's statement "it seems clear . . . that if we rule out the influence due to differences in experience and interest, the retention of girls is better than that of boys."[6] In the case of *Radium* the boys did, perhaps, have the advantage of greater interest. The other materials, however, probably had equal appeal. In view of the conflicting results and the small differences commonly observed between the sexes, it is difficult to accept as final Pyle's conclusions that "retention of girls is better than that of boys because of their more rapid development."[7] The correlation of the results of the boys and girls in the case of *Radium*, where the difference is most clearly marked, gives a biserial r of 0.196 ± .037.[8] This is obviously a very low relationship between memory and sex even though it is four times its probable error and even though the difference between the means is statistically reliable.

[6] *Op. cit.*, 150.

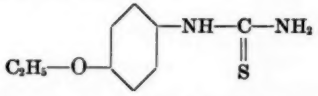
[7] *Op. cit.*, 146-151.

[8] For interpretation of biserial r see K. J. Holzinger, *Statistics for Students of Education*, 1928, 271-273.

A PRELIMINARY STUDY OF TASTE DEFICIENCY

By SAMUEL W. FERNBERGER, University of Pennsylvania

During the last several months a number of short papers and abstracts have appeared about the taste reactions and taste deficiencies of a complex chemical substance developed by Dr. Arthur L. Fox of the E. I. duPont de Nemours and Company laboratories. Fox calls the substance para-ethoxy-phenyl-thio-carbamide. It is the sulphur analog of dulcin, which is a substance described as about 300 times sweeter than sugar. The chemical formula of the substance in which we are interested is as follows:



As reported by Science Service,[1] Fox discovered that 60% who ingested this substance found it exceedingly bitter and the other 40% found it tasteless. No mention is made of the number of cases tested. Fox also reports what he calls 'taste-blindness' for other compounds in the thio-urea group. But everyone apparently finds dulcin in this chemical group sweet.

Blakeslee and Salmon publish the most comprehensive report on this problem.[2] Blakeslee prefaces this discussion with an observation on 'odor-blindness' made in 1927 which is not without interest. In his garden he had two verbena plants of different colors. On the basis of testing 40 persons he reports that 66% found plant A fragrant and B odorless while 33% found A odorless and B fragrant.

Using Fox's compound and working for taste thresholds Blakeslee and Salmon used 5 dilutions, the most dilute being tasteless to all subjects. They also used dry crystals. A total of 239 individuals were tested with the dilutions for threshold and 283 with the crystals. The primary interest of these authors was in testing families to discover a possible hereditary factor. They make, however, a number of general observations. The substance may sometimes be tasted but the taste is described as other than bitter. Working with the dry crystals it was found that of 152 men 38.2% and of 131 women 25.2% found it tasteless. Most of those who were able to taste it described the taste as extremely bitter. The authors say "the tables suggest that females are somewhat more acute tasters than the males but larger numbers will be necessary to show whether these differences are significant."

With regard to the question of inheritance, Blakeslee and Salmon find, from their analysis of family data, that the taste deficiency is apparently due to a single recessive gene. Their statement is emphatic. "Enough is known" they

*Accepted for publication January 18, 1932.

[1] *Science*, 73, 1931 (no. 1894), 14.

[2] A. F. Blakeslee and M. R. Salmon, Odor and taste blindness, *Eugenical News*, 16, 1931, 105-109.

write, "in regard to the inheritance of this specific taste 'blindness' to enable one to make predictions. If both parents are negative the children will all be negative. If one parent tests o (negative) and a child is found to be a taster, the other parent must be a taster."

Snyder,[3] on the basis of testing 100 families with this same compound, arrives at the same conclusions with regard to the inheritance of the deficiency. His percentages of tasters and non-tasters agree closely with those of Blakeslee and Salmon, being 68.5% and 31.5% respectively. Without giving his statistical basis in this report, he states that the taste deficiency "is not a matter of age, sex, nor race." This conclusion is not only at variance with the tentative findings of Blakeslee and Salmon with regard to sex but with regard to age as well.

Two more short reports of papers, read at the meetings of the National Academy of Sciences, have been published. Fox reports similar taste reactions in "a large number of related mono-aryl-thio-carbamides" and "symmetrical di-aryl-thio-carbamides."[4] Blakeslee,[5] continuing his threshold work, reports that "acuteness of taste for the chemical is inherited since there is an obvious correlation between the thresholds of parents and children." Suggestions are made for administering the dry crystals. Taste confusions for this and other substances are reported. Unfortunately the full reports of both of these papers have not appeared at the time of this writing.

In view of the results obtained with this compound, it seemed worth while to extend the experiment along psychological lines.[6] Dry crystals were administered to 1025 adults at the college student level. Of these 528 were men and 497 women. They were mostly students in psychology at the University of Pennsylvania and had a background of a discussion of fundamental taste qualities. They were told to swallow the substance, so as to stimulate the root of the tongue, and then report the experience in terms of one of the five following categories: tasteless, slightly bitter, bitter, very bitter, and extremely bitter. The category of 'tasteless' more or less defines itself; 'extremely bitter,' the other limiting category, was defined in terms of raw quinine, which unfortunately in these days is only known to less than half of the group. The middle term, 'bitter,' was defined in terms of the white of the rind of grapefruit. The *Os* seemed to have no difficulty in making judgments in terms of these categories. For the first 200 *Os*, litmus tests were made of the saliva before stimulation but this was discontinued inasmuch as there was obviously no relation between taste and the acidity or alkalinity of the saliva.

The results will be found in Table I. In order to more readily visualize these reactions the results are also produced as curves in Fig. I. The results rather closely conform to those of Blakeslee and Salmon and of Snyder with regard to the percentage of tasters and non-tasters, although our results are somewhat lower for non-tasters than both of the other sets. The relatively larger percentage of non-tasters among the men as compared with women, as reported by Blakeslee and Salmon, is confirmed by our results.

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[3] L. H. Snyder, Inherited taste deficiency, *Science*, 74, 1931, 151-152.

[4] A. L. Fox, The relationship between chemical composition and taste, *ibid.*, 607.

[5] A. F. Blakeslee, Genetics of sensory thresholds: Taste for phenyl thio carbamide, *ibid.*, 607.

[6] Dr. Fox kindly supplied me with the para-ethoxy-phenyl-thio-carbamide used in this experiment.

TABLE I

NUMBERS AND PERCENTAGES OF JUDGMENTS OF TASTE DISCRIMINATION

Judgment	Men		Women	
	No.	%	No.	%
Tasteless	158	29.9	116	23.3
Slightly bitter	72	13.6	30	6.0
Bitter	54	10.2	57	11.5
Very bitter	120	22.7	118	23.7
Extremely bitter	124	23.5	176	35.4
Total	528		497	

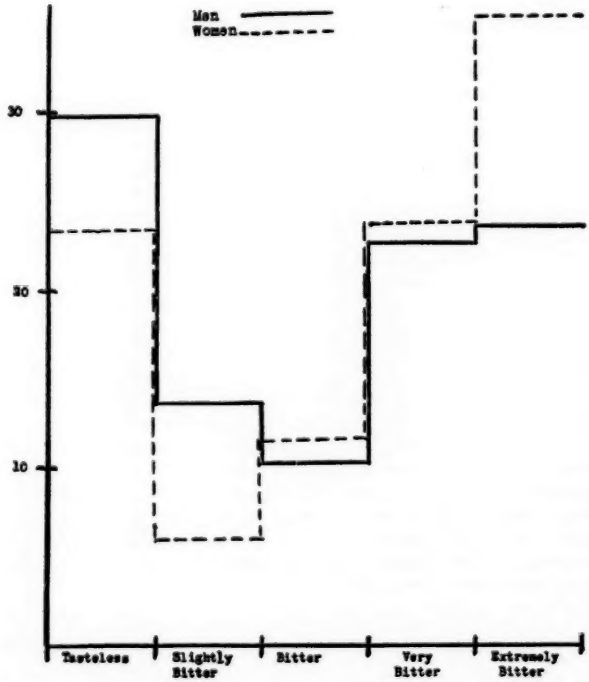

FIG. 1. PERCENTAGES OF JUDGMENT OF TASTE DISCRIMINATION FOR MEN AND WOMEN

An examination of the curves in the figure indicates, that for both men and women, they are bimodal. The percentages are high for 'tasteless' at one end and again for 'very bitter' and 'extremely bitter' at the other end. The percentages are low for both 'slightly bitter' and 'bitter' in the middle of the curves. Nevertheless one finds, what one would expect, that individuals exhibit all degrees of taste from excellent discriminations to total indiscrimination. Although the curves for men and women are of the same bimodal form still they are considerably and significantly different. The percentages for men are considerably higher at the indiscrimination end for both 'tasteless' and 'slightly bitter.' At the other end the percentages for women are invariably higher; only slightly higher for 'bitter' and 'very bitter,' but considerably larger for 'extremely bitter.' These differences in the form of the curves emphasize the sex difference to taste acuteness even more

TABLE II

FRACTIONATED NUMBERS AND PERCENTAGES OF JUDGMENTS OF TASTE DISCRIMINATION

	First Group				Second Group			
	Men		Women		Men		Women	
Judgment	No.	%	No.	%	No.	%	No.	%
Tasteless	104	30.8	49	22.8	54	28.4	67	24.6
Slightly bitter	53	15.7	17	7.9	19	10.0	13	4.8
Bitter	30	8.9	26	12.1	24	12.6	21	7.7
Very bitter	64	19.0	51	23.7	56	29.5	67	24.6
Extremely bitter	87	25.7	72	33.5	37	19.5	104	38.3
Total	338		215		190		272	

than the mere statement of the percentages of tasters and non-tasters. They amply verify the statement of Blakeslee and Salmon that women are more acute tasters than men.

In order to further verify this sex difference we have fractionated our results into two groups in the order in which they were taken. These results are given in Table II. Group I includes 553 *Os* (338 men and 215 women), and Group II includes 462 *Os* (190 men and 272 women). The results were obtained from classes and it seemed more proper to treat all results of a given class as a unit in its group even though the groups were of unequal size and the relative proportion of men and women was different. The curves for these two groups are very similar. In both groups the percentages for men are higher at the indiscrimination end for both categories of 'tasteless' and 'slightly bitter' and the percentages for women are higher at the other end for 'extremely bitter.' Inversions are found for the intermediate categories of 'bitter' and 'very bitter' but these middle categories are the only ones where the results are similar in any case for the two sexes.

It may be of interest to add the observation that a few *Os* noted taste confusions. These were classed as non-tasters when it was further ascertained that under no circumstances could the taste be described as bitter. These observations are too few to be given statistical treatment. The small number of such confusions may be due to the setting of our conditions with the definition of the four bitter categories which mitigated against the frequency of the reporting of other taste qualities.

All of the previous authors have spoken of the situation of indiscrimination of this compound as "taste blindness" apparently on the analogy of color-blindness. In certain respects the two are analogous: both involve indiscrimination of a particular sensory quality for some individuals while the same stimulus arouses intensive sensation in others; both show degrees of sensory weakness from full sensitivity to total indiscrimination and discrimination in both cases has a hereditary basis. The analogy would perhaps be complete except for two conditions. In the case of color-blindness to certain qualities it has been found that the indiscrimination covers all variations of the fundamental quality. If one is color-blind to red and green there is difficulty with *all* reds and greens. In the present case it is reported that individuals who find this substance utterly tasteless are nevertheless able to taste other bitters. Also in color-blindness there seems always to be a confusion of qualities. A given sensory quality is not seen as the *normal* quality but it *is* seen. And relatively few cases of such confusion are reported by previous workers or in the present experiment. Apparently in the case of this compound one either reports the taste of bitter or one fails to taste it at all. The reporting of some different taste seems to be relatively unusual. Indeed in this respect the situation seems more like that of the auditory indiscrimination of tonal islands.

But it would seem that the use of this and similar compounds offers an unexpectedly excellent opportunity for the psychological study of sensory indiscrimination. There are all too few cases of color-blindness and of tonal deafness. But here in taste approximately one-third of the people tested show indiscrimination.[7]

[7]Since writing the paper the following have appeared: A. L. Fox, The relationship between chemical constitution and taste, *Proc. Nat. Acad. Sci.*, 18, 1932, 115-120, and A. F. Blakeslee, Genetics of sensory thresholds: taste for phenyl thio carbamide, *ibid.*, 120-130. Fox, after investigating the taste of a large number of compounds of similar composition, comes to the conclusion that the taste of the C=S linkage is the basis of the bitter quality. He advances the theory for the failure of some individuals to taste this as due to the relative degree of solubility of the compounds in the saliva. Blakeslee, working with our present compound, determines the threshold values and finds that, for this compound, these have no relation to the thresholds for other bitters (quinine and picric acid) and for sour, sweet and salt. There is also no relation between the bitterness and the acidity of the saliva as measured by the hydrogen ion concentration.

A COMPARATIVE STUDY OF THE ERRORS OF LOCALIZATION ON THE FINGER-TIPS

By KARL M. DALLENBACH, Columbia University

This study is intended as a check or control of the experiment on localization that Professor Harry Helson and the writer are performing in connection with a study of nerve regeneration.

In 1924 the writer cut all the nerves, subcutaneous as well as cutaneous, that led to the radial half of the first phalanx of the left index finger.[1] Study of the errors of localization was one of the ways in which regeneration of the nerves and return of sensitivity were investigated. As the incision was accidental and the experiment was not undertaken until after that event, we could not obtain the normal error of localization for the experimental tissue. Since some idea of the normal error was necessary for the precise evaluation of the results, we attempted to gain the desired information indirectly by studying the errors of localization on normal, comparable tissues. We, therefore, repeated the localization experiment upon similar areas on the right index and left great fingers.

The radial side of the first phalanx of the right index finger bears the closest resemblance anatomically to the experimental tissue; but experiments upon it alone would not yield a norm which we could accept as representative of the tissue on the left index, because *O* is right-handed. His right-handedness may mean (1) that the right index finger is more sensitive than the left—in which case the norm obtained from the right would be too small for the left; or (2) that the localizations, since they were made with *O*'s left hand, are less accurate and precise than the right-handed localizations made on the left index—in which case the norm obtained would be too large. The extent to which these opposite and antagonistic factors are effective can not be known short of a check or control experiment upon *O*s with normal tissues.

We sought, nevertheless, to eliminate one of these factors—differences in the accuracy and precision of localization caused by handedness—by repeating the experiment upon the radial side of the first phalanx of *O*'s left great finger. Here *O* made his localizations, as he had made them on his left index, with his right hand. Still, the norm obtained here can not be accepted as representative of the experimental tissue because of possible differences in the sensitivity of the two areas. How the radial areas of the left index and left great fingers differ can again not be known until a series of control experiments have been made.

Problem. The problem of the present study is a comparative one. We wish to know how, with normal skin tissue, the errors of localization upon the radial side of the first phalanx of the left index, the right index, and the left great finger compare.

*Accepted for publication January 11, 1931.

[1]For the localization and extent of the incision, cf. K. M. Dallenbach, The bidimensional reproduction of the surface of a tridimensional object, this JOURNAL, 43, 1931, 127.

Observers. Six *Os*—3 men (*A*, *B*, and *C*) and 3 women (*D*, *F*, and *G*)—served in this experiment. All were undergraduate students of Cornell University and members of the class in qualitative laboratory psychology. They had all completed the routine localization experiment at the time that they served in this study.

Method and procedure. The errors of localization were determined for every one of the *Os* for the radial side of the first phalanx of (a) the left index finger, (b) the right index finger, and (c) the left great finger. The usual procedure of localization experiments was followed. *O* was blindfolded and seated in a comfortable position at a table, with the experimental area held in a fixed position by a plaster-cast hand-rest. *E* then marked with ink 10 spots, distributed haphazardly over the experimental area. Every spot was stimulated with an aesthesiometer in chance order, but in turn, 10 times. The point of the aesthesiometer, which was rounded and made of gutta-percha, was 1 mm. in diam. It was applied after a 'ready,' 'now' signal, on the all-or-none principle, with a pressure of 12 grm.; and it was allowed to rest upon the skin for approximately 1 sec.

As soon as *E* removed the stimulator, *O* tried to locate the spot touched, using a gutta-percha pointed stylus, which was similar in weight and dimensions to the aesthesiometer used by *E*. He was allowed to move it freely over the surface of the skin until he was satisfied with his localization. He then pushed gently upon the stylus so as to leave, when the stylus was removed, a slight indentation. This mark was used by *E* in measuring the error of localization. Measurements were made by *E* with the aid of dividers and a millimeter scale.

Before every experiment *E* brought *O*'s stylus, which was held in *O*'s free hand, to a fixed point upon the table. The gross movements that *O* had to make in localizing the spots were consequently the same in every experiment.

The experiments (100 in number) upon an area were completed during a single observational period. The order in which the different experimental areas were considered was varied, so as to reveal the effects of practice, if any, for the different *Os*. The order for *A* and *B* was left index, right index, and left great; for *C* and *D*, right index, left great, and left index; and for *F* and *G*, left great, left index, and right index. Every area was, therefore, experimented upon as frequently in the first as in the second and third positions.

The experiments were conducted on all the *Os* at the same hour of the day, *i.e.* between 2:00-4:00 P.M.

The extent and direction of the error of localization, *i.e.* whether the error was peripheral (P), central (C), dorsal (D), ventral (V) or intermediate between these cardinal points, was recorded by *E* in the gross data sheets.

Results. (a) *Extent of the average error.* Table I gives, for every *O* and for every experimental area, the average error of localization together with its m.v. It shows that the average is, for all the *Os* except *G*, smallest for the left index finger, largest for the left great finger, and intermediate in size for the right index finger. For *G* the left index finger gives the largest average error, the right index the smallest, and the left great finger an average error intermediate in size.

In spite of the approximate uniformity of these results, the differences among these experimental areas (shown in Table II) are so small, and the m.v. of the averages (shown in Table I) are so large —though not larger than is usual in work of this kind—that the mathematical significance of the differences is very slight.

TABLE I

THE AVERAGE ERROR OF LOCALIZATION WITH ITS MEAN VARIATION FOR EVERY *O*, FOR EVERY EXPERIMENTAL AREA

(The rank of the different averages among the three for every *O* is given in parenthesis after the value)

		Experimental Area		
O	Sex	Left Index	Right Index	Left Great
A	M	0.94± .58 (1)	0.97±.61 (2)	1.17± .56 (3)
B	M	1.38± .81 (1)	1.46±.64 (2)	1.62± .95 (3)
C	M	1.61± .82 (1)	1.66±.96 (2)	1.74±1.10 (3)
D	W	1.85± .53 (1)	2.09±.60 (2)	2.23± .52 (3)
F	W	1.62± .32 (1)	1.68±.26 (2)	1.72±1.00 (3)
G	W	1.83±1.30 (3)	1.63±.70 (1)	1.68±1.00 (2)

TABLE II

THE DIFFERENCES, FOR EVERY *O* AND FOR EVERY EXPERIMENTAL AREA, AMONG THE AVERAGE ERRORS OF LOCALIZATION

	Difference between		
O	Left Index and Right Index	Left Index and Left Great	Right Index and Left Great
A	−.03	−.23	−.20
B	−.08	−.24	−.16
C	−.05	−.13	−.08
D	−.24	−.38	−.14
F	−.06	−.10	−.04
G	+.20	+.15	−.05

The greatest differences among the average errors of localization are shown by *D*, and the m.v. of her averages are among the smallest obtained. Her data, of all our data, are, therefore, the most favorable for showing a difference among the experimental areas used in this study. Computation of the significance of her differences yielded the following results. The difference between the average errors of localization on the left and right index fingers is 0.24 mm., the σ diff. is 2.30, and the D/σ diff. is 0.10; the difference between the left index and the left great finger is 0.38 mm., the σ diff. is 2.44, and the D/σ diff. is 0.15; and the difference between the right index and the left great is 0.14 mm., the σ diff. is 2. 33,and the D/σ diff. is 0.06. These data mean that these differences might be expected to occur 54, 56, and 52 times, respectively, in 100 trials—*i.e.* practically with chance frequency. Since *D*'s data do not show reliable differences, we feel justified in concluding that the differences of the other *O*s are likewise due to chance.

The results of this study indicate, therefore, that the average error of localization obtained from any one of the three experimental areas used in this experiment may be used as norms for the other two.

TABLE III

THE NUMBER OF TIMES, FOR EVERY *O* AND FOR EVERY EXPERIMENTAL AREA, THAT AN ERROR WAS MADE IN EVERY ONE OF THE CARDINAL DIRECTIONS

(Italics show for each dimension the direction giving the greater number of errors)

Area	*O*	Direction			
		P	*C*	*D*	*V*
Left Index	*A*	*41.5*	20.0	6.5	*22.0*
	B	*32.0*	17.5	18.0	*23.5*
	C	22.5	*52.5*	6.5	*23.5*
	D	5.5	*42.0*	1.0	*41.5*
	F	26.0	*27.5*	6.0	*26.5*
	G	*35.0*	13.5	9.5	*20.0*
Right Index	*A*	*32.0*	11.5	7.5	*33.0*
	B	*37.0*	11.5	17.5	*23.0*
	C	10.0	*29.5*	7.0	*43.5*
	D	11.5	*42.5*	12.0	*23.0*
	F	16.5	*37.0*	12.0	*21.5*
	G	*46.0*	10.5	9.5	*15.0*
Left Great	*A*	*37.0*	20.5	12.5	*19.0*
	B	*38.5*	11.0	19.0	*21.5*
	C	21.0	*29.5*	17.0	*19.5*
	D	5.0	*61.0*	10.5	*15.5*
	F	19.5	*34.5*	10.5	*21.5*
	G	*36.5*	16.0	7.0	*26.5*

(*b*) *Direction of the error.* Table III gives for every *O* and for every experimental area the number of times an error was made in the peripheral, central, dorsal, or ventral direction. The gross data sheet divided the errors into 8 groups, the cardinal directions mentioned and the 4 intermediate ones, *i.e.* PD, PV, CD, and CV. The data were, however, consolidated so as to bring out more clearly the directional aspects of the errors of localization. The consolidation was accomplished by dividing the number of errors listed under the intermediate directions between the two cardinal directions concerned; for example, if 11 PD errors were indicated for an *O*, these were divided equally between P and D, *i.e.* 5.5 errors were added to the peripheral and dorsal errors.

Three of the *O*s (*A*, *B*, and *G*) made more peripheral than central errors in their localizations in all of the experimental areas, and three (*C*, *D*, and *F*), contrariwise, made more central than peripheral errors in all of the areas. These results show that central or peripheral disposition is an individual matter, and, furthermore, that as an individual is disposed in one of our experimental areas he will very likely be disposed in the others.

All of the *O*s made, in all of the experimental areas, more ventral than dorsal errors. The constancy of this result may be due to the fact that the *O*s always approached the experimental areas in making their localizations from a ventral

direction. *E*, it will be recalled, placed *O*'s stylus before every experiment at a fixed position which was ventral to the experimental area. Whether this is the explanation of the predominant ventral tendency shown in our results, future experiments alone can determine. It suffices for the purpose of this study to note that there is no difference in this respect among our experimental areas. A ventral error in one of our experimental areas is correlated with ventral errors in the others.

Conclusions. From the results of this study we may conclude that there are no reliable differences either in extent or direction of errors of localization among the experimental areas used in this study. The norm obtained from one of them will serve as a norm for the other two.

We may, therefore, in the localization test of nerve regeneration that Professor Helson and the writer are making, safely use the average values from our *O*'s right index and left great fingers as norms for his left index finger.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF NEBRASKA

V. Phenomena Accompanying the Inhibition and Control of Breathing

By Rudolf Vogeler and J. P. Guilford

A previous study was concerned with the ability of the average subject to inhibit breathing and to breathe slowly, and with the rate of learning either to inhibit or to control breathing.[1] Two groups of *S*s were trained for an hour a day over a period of 15 days, and kymograph records were kept of every minute of practice. These records reveal several interesting phenomena which seem worth reporting. The experimental procedure was fully described in the previous report and so will not be repeated here.

The present study deals with the following questions to which our data give at least a partial answer:

(1) What is the difference, if any, in the return to normal breathing after holding the breath, and after slow breathing?

(2) What is the relation between the breathing phase at the signal to 'hold' and the ability to hold the breath? In other words, does 'holding' at different stages of the inspiration-expiration cycle have an effect upon the length of the hold?

(3) What is the difference, if any, in the physiological consequences due either to 'holding' or to 'slow breathing'?

(4) What factors, favorable and unfavorable, influence 'holding' and 'slow breathing'?

An examination of the kymograph records shows at once (a) that immediately after the end of a period of inhibition or of slow breathing, there is always a compensatory period of very deep breathing and a more or less gradual return to normal breathing; and (b) that there is a distinct difference between the two cases, inhibition and slow breathing.

The curves designating the return to normal were measured as follows. Five series of breathing curves which appeared to be typical for every *S* were chosen from the kymograph records obtained on different days. The same general appearance of the curves chosen was exhibited throughout the entire training period. Each one of the first fifteen successive exhalations in each series was measured from a line bisecting the curves, approximately in the center, to the peak of each curve. Only the exhalations could be measured because the entire amplitude of these curves could not be recorded. During very deep breathing, the lever of the recording apparatus was stopped by the edge of the disk of the tambour, thus

*Accepted for publication February 15, 1931.

[1] R. Vogeler and J. P. Guilford, Learning to inhibit and control breathing, this Journal, 43, 1931, 624-630.

preventing the registration of the complete inhalation. We have assumed that the exhalations exactly balance the inhalations, and the records tend to show that this assumption is correct. By averaging each curve of the five series chosen, the differences in height for the fifteen successive peaks were obtained. Then the averages for each group of 5 *Ss* were computed and plotted as in Fig. 1.

Fig. 1 shows the average height of each curve in each series. One *S* in each group showed such a distinct departure in the size of the first few exhalations as compared to the other *Ss* in the group, that these averages were not included.

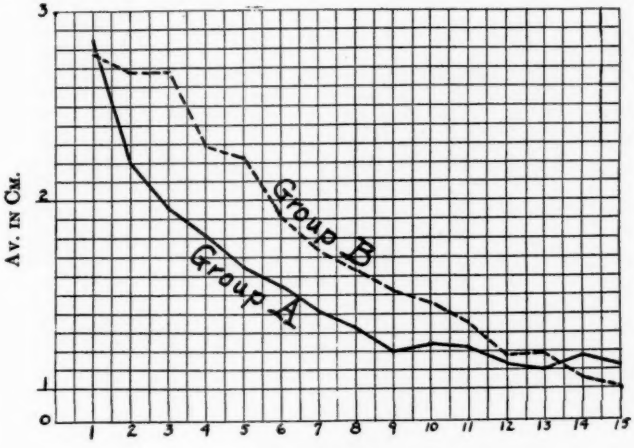

Fig. 1. Return to Normal Breathing
Group A after 'holding'; Group B after 'slow breathing'

As it is, averaging the results of 4 *Ss* tends to smooth out the differences between the two groups, because some *Ss* tend to return to normal more quickly than others. It was difficult to measure the exact center of each series and consequently only the relative differences are obtainable.

Fig. 1 indicates a decided difference between the two groups. The curve for Group A (the *Ss* who held their breath), which starts above that for Group B, drops very rapidly at the start and begins to flatten out at the ninth breath. The curve for Group B (the *Ss* who breathed slowly) drops more gradually at first, then more rapidly starting at the fifth breath, beginning to flatten out at the twelfth breath. Group A apparently comes back to normal sooner than Group B, as indicated by (1) the more rapid initial descent, and (2) the more rapid flattening out of the curve. That the above is a true description of the two types of returns to normal for both groups is borne out by the fact that the individual curves show the same phenomena in every case.

The time element may be the factor causing this difference, since 'slow breathing' practice periods lasted longer, comparatively, than 'holding' in most cases.

The longest 'holding' period lasted 93.3 sec., whereas the longest 'slow breathing' period lasted 746 sec. In other words, the amount of work expended by Group B was greater than that of Group A, thus demanding a greater amount of subsequent ventilation.

It is interesting to note that this greater ventilation in Group B compensated for the rapid decrease in the curve of Group A, thus enabling both groups to approximate normal breathing at about the same time. But the continued decrease in ventilation for Group B continues beyond the point of normal breathing! This

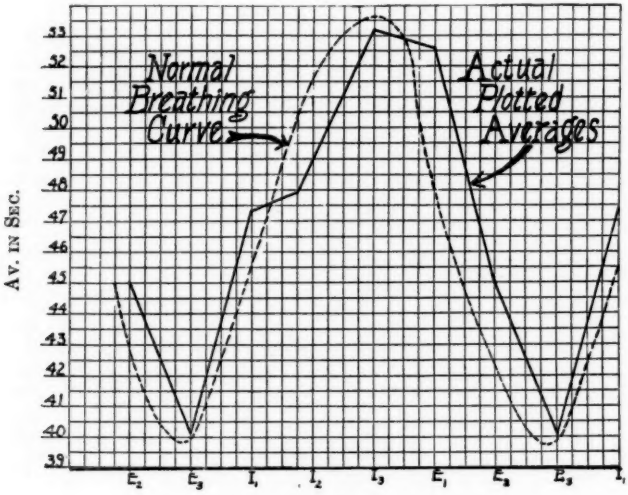

FIG. 2. RELATION OF THE BREATHING PHASE TO THE LENGTH OF THE 'HOLD'

(I_1 represents the first third of inspiration; I_2, the middle third; and I_3, the last third; E_1 represents the first third of expiration; E_2, the middle third; and E_3, the last third.)

seems to indicate a 'carry-over' effect of 'slow breathing' practice to continue beyond the normal. This phenomenon is brought out by the kymograph records in several cases. Some of the introspections bore this out also. For example, one *S* reported that "slow breathing becomes almost automatic"; another *S* that during the day he found himself "taking a deep breath" as though he "were not getting enough air." The latter phenomenon might certainly indicate an automatic return to 'slow breathing' followed by a deep compensating inhalation.

The relation between the breathing phase at the signal 'hold' and the ability to 'hold' proved to be very interesting. This relation was determined as follows: the entire breathing cycle was divided into six parts, during any one of which the signal might have been given.

In Fig. 2 the dotted line represents the breathing cycle. I_1 represents the first third of the inspiration, I_2 the middle third, and I_3 the last third or the point of greatest inspiration, E_1 the first third of the expiration, E_2 the middle third, and E_3 the last third or the point of greatest expiration.

Upon examination of the kymograph records it was determined at which of these six phases the 'hold' began. Reverting to the daily records, the lengths of this 'hold' were tabulated. There were 77 measurements for I_1, 35 for I_2, 112 for I_3, 39 for E_1, 58 for E_2, and 45 for E_3. All the measurements for each phase were averaged. The results show that the length of the 'hold' is directly proportional to the amount of air in the lungs.

It is interesting to note that the plotted averages, (see Fig. 2), form a curve which approximates very closely the normal breathing curve. The rise of the curve is gradual, corresponding to the inspiration in normal breathing, whereas the fall is sharper, corresponding to the more rapid expiration. From the appearance of the curve it will be noticed that the plotted points denoting the last third of the inspiration and the first third of the expiration are almost equal. At these points the amount of air in the lungs is approximately the same.

It has been shown previously that there is no relation between vital capacity and normal breathing, nor any relation between the vital capacity and the return to normal.[2] Nevertheless, there is a distinct difference in the physiological results obtained due to 'holding' and 'slow breathing' as reported by the *Ss*. The most outstanding of them for Group A were as follows: (1) oppression in the chest region, (2) feeling as though lungs and diaphragm were fluttering, (3) accentuated throbbing of the heart, (4) yawning and drowsiness, (5) soreness in the chest region. For Group B they were: (1) fatigue in the abdominal region, (2) contractions in the abdominal region, (3) increased bodily temperature, (4) restlessness, (5) sensation of choking. The effects, regarded collectively, are indicative of asphyxia in Group A, and of fatigue in Group B.

The connection between 'holding' and asphyxia, and between 'slow breathing' and muscular exertion and effort, may be made more convincing if the two cases are discussed separately. The 'holding' group will be treated first. The oppression in the chest region would seem to indicate that the stimulus for the breathing reflex is located to some extent, at least, in the peripheral organs. The impressions of 'fluttering' lungs and diaphragm may be real abortive automatic attempts to breathe. The accentuated throbbing of the heart is apparently a compensatory measure to make up for a vanishing source of oxygen in the lungs. There is little doubt that the prolonged inhibition with its accompanying deprivation of oxygen caused subsequent yawning and a feeling of drowsiness. The "soreness in the chest region" still remains to be explained. It must be remembered that the complete inhibition, which is being discussed, is in some respects the same as maintaining a postural position for a long time, in which the muscles assume a state of static contraction. Upon the strength of this explanation the "soreness" is a real fatigue-effect, but it is more properly regarded as an after-effect, not as an accompanying factor.

In 'slow breathing' the factor of "fatigue in the abdomen" is reported, but not described very fully. If the analogy of 'slow breathing' to a working muscle is valid, the fatigue is easily explained. This analogy is strengthened by the fact

[2] *Op. cit.*, 626.

that the bodily temperature was apparently increased as shown by the appearance of such physiological indications as perspiration and flushing of the face, probably due to an increased rate of oxidation. A factor similar to the 'diaphragm fluttering' of Group A is found in Group B in which "contractions in the abdominal region" were reported. This seems to be merely an attempt of the diaphragm to react in its rhythmic way, but through the voluntary control the automatic or reflex excitation is inhibited immediately after or soon after it has started. It should be mentioned that this phenomenon was reported oftener in Group B than in Group A.

The factor of 'restlessness' such as the moving of hands and feet, seems to indicate an innervation of the surrounding musculature in the attempt to breathe adequately. This is still further brought out by the reports of several *Ss* who claimed that 'strained feelings' were spread into the arms and back, causing them at times to shift their posture. The symptom of sensations of choking is somewhat more indicative of asphyxia than of fatigue, however.

Our previous discussion of the relation between the breathing phase and the length of the 'hold' certainly bears out the fact that "starting with full lungs" is a favorable factor. The reports that "reading interesting material" and "forgetting about the breathing" were favorable factors, are inter-related. Bair's work on voluntary control shows the advisability of removing the attention from the movement to be effected.[3] This makes for relaxation which in turn results in a greater ability to inhibit. This strengthens the conclusions to be drawn from the report on physiological results in which relaxation seemed to be a favorable factor.

The analogy of 'slow breathing' to a working muscle is strengthened by the report of the *Ss* that "fatigue in the abdominal region" was an unfavorable factor. One *S* went so far as to report that "it is not a lack of air, but rather fatigue in the abdomen" which made him stop "breathing slowly."

The mental attitude of *S* helped very much to determine the results on many occasions. The presence or absence of determination and confidence, for example, were to some extent responsible for a corresponding increase or decrease in the records.

Eating shortly before the practice session usually resulted in a decreased average for that session. The rate of metabolism being increased under these conditions, a decrease in the supply of oxygen could not be maintained for a very long time. In other words, an additional load above the average was being carried by the inhaled oxygen in fulfilling its additional work of oxidation during the process of digestion. The pressure exerted by the full stomach upon the diaphragm is another factor which must be considered under these circumstances.

Consciously or forcibly expanding and contracting the thoracic and abdominal regions while 'holding' was reported as favorable for a longer 'hold.' This might make it possible to use to maximum advantage the residual air in the lungs; or it may be that a voluntary action inhibits a reflex which it resembles or which employs the same muscle groups.

For 'slow breathing' two individual devices were reported: (1) tensing the abdominal region, and (2) smoothing out the breathing. The 'tensing' seems to be a conscious attempt to inhibit the reflex or rhythmic contractions which were

[3] J. H. Bair, Development of voluntary control, *Psychol. Rev.*, 8, 1901, 474-510.

felt at different times during the practice. "Smoothing out the breathing" is for the 'slow breathing' group what 'relaxation' is for the 'holding' group. Further experimentation using designated types of favorable devices should prove interesting and more conclusive.

Conclusions

(1) The return to normal breathing after holding the breath shows a very rapid initial drop, the curve flattening out at about the ninth breath, whereas the return to normal after breathing slowly shows a more gradual curve, the rapid fall starting at about the fifth breath and the curve flattening out at about the twelfth breath.

(2) The length of time one can hold the breath is directly proportional to the amount of air in the lungs.

(3) The physiological effects of holding the breath approximate asphyxia, whereas the effects of breathing slowly approximate fatigue.

(4) Favorable factors for holding the breath include (a) starting with full lungs, (b) diverting the attention from the breathing, (c) mental set, such as determination and confidence, and (d) forcibly expanding and contracting the thoracic and abdominal regions. Unfavorable factors include the converse of the above plus the factor of eating just before the practice session.

(5) Favorable methods reported for 'holding,' were to relax generally and to tense the abdominal region; and for 'slow breathing,' to smooth out and to prolong the respiratory activities.

APPARATUS

AN AUTOMATIC TECHNIQUE FOR ESTABLISHING CONDITIONED REFLEXES

ROGER BROWN LOUCKS,[1] Johns Hopkins University

A serious limitation of the usual conditioned-reflex techniques is the small number of animals which can be used by a single experimenter. To increase efficiency, a system has been developed in which the administering of stimuli and food and the recording of reactions are entirely automatic. Advantages of this procedure are: (a) that it offers the possibility of working simultaneously with several dogs (*i.e.* where two or more sound-shielded rooms are available); (b) that training which involves merely the repetition of stimuli will not necessitate the experimenter's concentrated attention but will leave him relatively free for other work; and (c) that, when the need arises, such training can be left to the supervision of a laboratory assistant.[2]

Motor-driven timer or circuit-closer. It is frequently desirable in conditioned-reflex investigations to utilize rhythmical stimuli, *e.g.* a flashing light, an intermittent tactual prick, or a metronome beat. In Pavlov's laboratory the first of these stimuli is produced manually by closing a switch at the desired rate; and the intermittent tactual stimulus is administered by pressing a bulb. Pneumatically started metronomes provide rhythmical auditory stimuli, separate instruments being used for different rates. In the present arrangement, all intermittent stimuli are controlled by one device, a motor-driven timer. A brass disk driven by an electric phonograph motor is studded with small rivets arranged in concentric circles.[3] A small rider can be set to trail over rivet-heads in any one of these circles. Each time the rider passes over a rivet-head it is lifted, thereby causing two flexible strips to touch. In a circle with 30 rivets, 30 contacts are made per revolution, and if a light is connected in the circuit it will flash 30 times. (See Fig. 1, B.)

[1]National Research Fellow in the Biological Sciences. This technique has been worked out in the Conditioned Reflex Laboratory of the Henry Phipps Psychiatric Clinic, Johns Hopkins Medical School. The writer is greatly indebted to Dr. Adolf Meyer for extending the facilities necessary to carry on the project. He is also greatly indebted to Dr. W. Horsley Gantt for clarifying numerous questions regarding Pavlov's training procedures in the light of Dr. Gantt's intimate contact with the Russian work. Dr. Gantt, who established the Laboratory, has elsewhere described the sound-shielded room. The present paper should be read in conjunction with Dr. Gantt's more comprehensive description (forthcoming).

[2]In Pavlov's laboratory, work is carried on with the aid of a large number of medical students who train animals for credit toward a degree. Pavlov, or more accurately, one of his principal collaborators, takes these students in hand and sets them to work with one or two dogs on a specified problem. At the end of the training period, Pavlov comes and spends perhaps as much as a fortnight in personally verifying the results.

[3]A synchronous motor was not used because the city current was not at first telechronized.

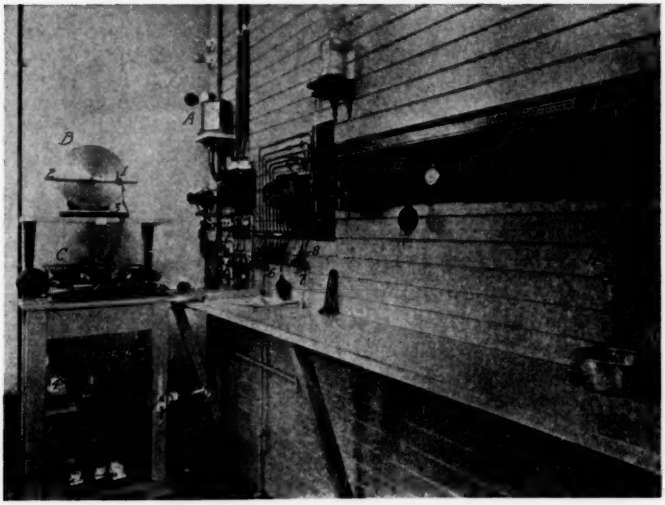

FIG. 1. GENERAL VIEW OF APPARATUS

A. Time-clock.

B. Motor-driven timer or circuit-closer.
General Electric induction disk phonograph motor, geared down to 2 r.p.m.
1. Two strips, the outer of spring steel, the inner of brass, have tungsten contacts on their adjacent surfaces. The circuit is closed when these contacts touch. No current passes through the disk.
2. Brass bar upon which the rider slides and is clamped.
3. Rubber sponges for sound-insulation. The revolving disk is 10 in. in diam. Distance between rivet-centers is the same in all circles. The smallest circle has 10 rivets, and in 1 1/4 in. in diam. The largest circle has 80 rivets.

C. Gaertner 4-pen tape-recording chronograph supported on sponge-rubber.

D. Vacuum-tube oscillator or tone-generator.
4. Oscillator-switch consisting of a lamp-socket filled with mercury and an electric door-bell with gong removed. When the clapper of the bell dips into the mercury, it closes the oscillator's B-battery circuit.

E. Vacuum-tube relay for time-clock.
5. Philco relay, z 412, rewound to original diameter with No. 27 wire.

F. Shield about observation port-hole.
6. Pipe-line conveying compressed air. The solenoid valve (CR 9505), normally closed, is not in view but is fully described in General Electric catalogues. The valve was placed in a celotex box.
7. Pipe-line coming from 'thigmoton,' in basement. The greater part of this line is 1/4 in. rubber-hose with a 5/16-in. wall.
8. Individual stop-cock valve.

G. Board with scale for measuring secretions.
A bubble of air is introduced into the system and its displacement measured on the scale.

H. Dropper.
Spacing of electrodes may be adjusted by thumbscrew. Vacuum-tube relay not shown in photograph, but see Fig. 4. The relay is a D.C. clapper-type made by Westinghouse, S 695,633. The heater supply for the cathode is omitted from Fig. 4; it may be either A.C. or D.C.

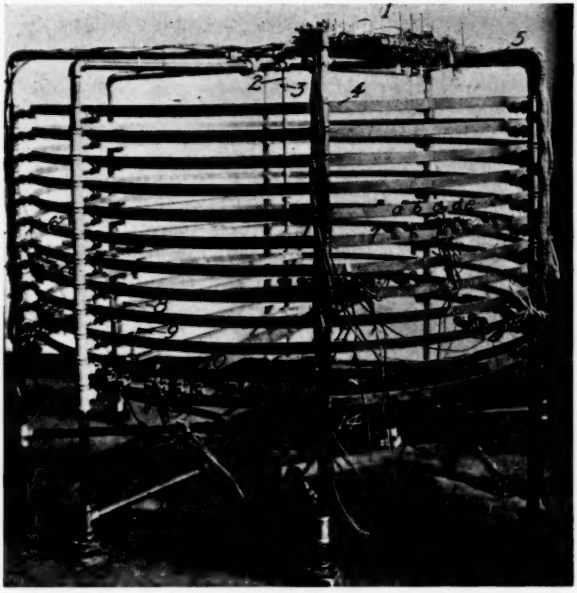

FIG. 2. MASTER-CONTROL OR ROBOT

1. Switchboard. Wires from apparatus terminate in upright brass binding-posts. In order to shift a particular stimulus it is necessary to change two small battery-clips from one pair of posts to another.
2. Shaft. Note that the shaft is fixed and that the arms turn.
3. Collar fixed to shaft provides bearing for arms which trip switches.
4. Spring steel hoops which support toggle-switches.
5. Cable carrying wires from apparatus at sound-shielded chamber.
6. Roller, at tip of arm, actually tripping toggle-switches.
7. Below the letters a, b, c, d, e, f are 6 toggle-switches which control events connected with the administering of one stimulus. Switch a, when tripped, closes the circuit starting the tape recorder: f opens the recorder-circuit, stopping the chronograph motor. Similarly, switch b starts the stimulus, and e shuts it off. Switch c closes the food-box circuit and d breaks it.
8. Channel arm. This is bolted to a cast-iron pillow-block which turns on the shaft, bearing upon the collar noted in 3.
9. Angle-iron catching arm directly above.
10. Angle-iron connected with driving-gear by means of a ratchet and pawl. This arm does not trip switches but merely serves to pull the other arms around.
11. Boston speed-reducer LB 400, geared directly to motor. The output side of the reducer is geared to a 50-tooth worm-gear.
12. Nob on toggle-switch in tripped position. Distance from centerline of shaft to toggle switch nob is 2 ft. 3-3/8 in.
13. Wooden platform carrying motor and gear-reduction box.
14. General Electric D.C. 1/4-hp. compound-wound motor. (Again a synchronous motor would have been preferable.)
15. Iron-pipe framework for supporting spring-steel hoops.
16. Radial iron pipe-brace of general frame-work.
17. Rubber hose for insulating sound.

Apparatus for administering tactual stimuli: "Thigmoton." Tactual stimuli are given by an electromagnetic-pneumatic apparatus at a rate controlled by the motor-timer described above. Closing the electrical circuit in the experimental room causes current to flow through a solenoid in a room on the floor below. When the plunger of the solenoid is actuated it causes the rubber dam of a tambour to be struck. The wave of compression resulting from this blow passes up a rubber hose to the sound-shielded experimental chamber. By opening the proper stop-cock valve, the pressure wave can be directed into any one of 7 mechanical prickers fastened to the dog's side. These consist of brads set in solder and glued to the rubber dam of a small tambour. The tambour is supported in an aluminum frame which is stuck on the dog's side by means of a low-temperature wax.

Methods of producing sound: (*a*) *Clicks.* The motor-timer is used in conjunction with a telegraph sounder to produce a variety of 'metronome' clicking rhythms.

(*b*) *Bubbles and hisses.* A magnetic valve is connected with a compressed-air line. When the electrical circuit is closed, the released air passes through a conduit into the sound-shielded chamber and audibly bubbles through water or hisses from the orifice of a pipe, according to the setting of stop-cock valves. When a bubbling noise is desired, the stop-cock valve leading to the bottle of water is manually opened before closing the magnetic-valve circuit.

(*c*) *Tones.* Tones are generated by a very inexpensive vacuum-tube dynatron-oscillator suggested by Lampkin's circuit.[4] The tuning inductances consist of a Western Electric 5B Auto-Transformer with 12 taps, which provides the middle range of frequencies, and a honeycomb coil #1250, which supplies the higher tones. Fixed condensers ranging in steps from 0.001 to 0.5 mfd can be switched into the circuit either singly or in combinations. Pitch is shifted by altering either the inductance or the capacity. Volume can be varied independently of pitch. Baldwin ear-phones are connected through a 500,000-ohm potentiometer to the output, one receiver being placed inside the sound-shielded chamber, the other near the experimenter. As now constructed, the oscillator has a range from approximately 32 d.v. to a value well above the upper limit for man. The higher tones appear satisfactory as regards freedom from harmonics; and it seems probable that, with a dynamic-type telephone-receiver to transmit the tone, the lower tones, which now carry an undesirable number of overtones, will be improved. Further details will be given as studies utilizing the tone-generator are published.

Food-box. The food-box now in use consists of 12 hinged shelves mounted one above the other in a chute. On a level with each shelf is an electric door-bell with the gong removed. The clapper arm or armature supports the shelf at its outer edge. When the coil of the bell is energized the armature is attracted and the shelf drops.

Tape-recording chronograph. A Gaertner 4-pen tape-chronograph, which marks on a waxlike-coated paper, is used for recording. It is imperative that no part of a record be illegible; and this instrument has proved singularly reliable. The range of speeds was shifted by adding a light in series with the motor. Click-

[4] G. F. Lampkin, A dual-frequency audio source for general laboratory use, *Electronics*, 1, 1930, 417.

ing of pen-armatures was reduced to a minimum by placing rubber washers on the pole-pieces. One pen records the duration of the stimulus, a second indicates the point at which food is given, a third the drops of saliva secreted by the dog, and the last, the time in seconds.

Time-clock. When the frequency of the city current was telechronized a common electric house-clock was fitted up to replace the Harvard time-clock, notoriously erratic, which had been employed thus far. This device is very dependable; and the marker can be started by merely throwing a switch.[5] A brass gear with sixty teeth was turned down to 1/16-in. in thickness, and the hub re-

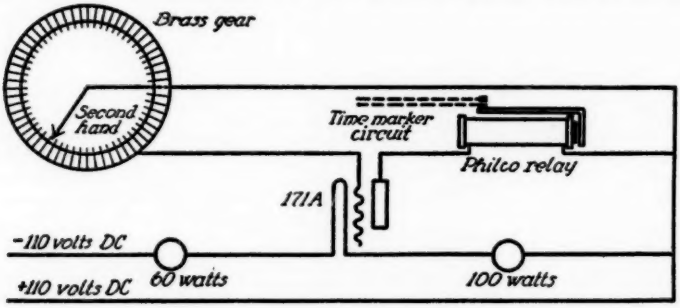

FIG. 3. CIRCUIT OF THE TIME-CLOCK

moved. It was then fastened by sealing wax to the inner surface of the clock's glass cover. A coil-spring was soldered to the second-hand so as to touch lightly the teeth of the gear, thus making sixty contacts per revolution. The circuit in Fig. 3 indicates that at each contact the grid current of the vacuum tube is increased: the coincident increase in plate current actuates a relay. The Philco Relay, rewound with #27 wire to increase the impedence, can be omitted where it is feasible to rewind the marker.

Drop-recorder. For the past eleven months, records of salivary secretion have been taken by means of a very simple but reliable vacuum-tube drop-recorder. This method of recording differs from that of Hannicke, in Pavlov's laboratory, in that no electrolyte is necessary and that drops are removed from the electrodes by surface-tension rather than by suction. Furthermore, in the Russian arrangement the conduit is filled with air as far as the electrolyte-bottle, whereas in the present system the pipe is entirely filled with water. Secretion of saliva displaces water in a conduit connecting the glass receptacle fastened over the dog's parotid fistula with a dropper outside the sound-shielded chamber. See description by Gantt (note 1).

Recently, Harris has described the vacuum-tube drop-recorders used by English investigators.[6] The open-grid circuit described in the present paper

[5]This device is adaptable for large laboratory classes, the only modification necessary being the addition of a Philco Relay to each group of student desks. Philco Relays cost 75c, the vacuum tube an equal amount, and electric clocks can now be purchased for as little as $1.00.

[6]D. T. Harris, Thermonic drop-recorder, *J. Physiol.*, 71, 1931, xxii.

(Fig. 4) is particularly suitable where the dropping rate varies over a wide range, as is the case with conditioned secretions. Due to the fact that each drop decreases the plate current through the relay solenoid, thereby releasing the armature, the excursion can be very rapid and slight, and yet break the marker-circuit effectively. Since the grid bias is an all-or-none affair, the adjustment is not critical and does not require attention.

The orifice of the dropper is a glass pipe drawn to 0.977 mm. outside diameter. The upper electrode, a wire .202 mm. diam., is brought into the pipe through an

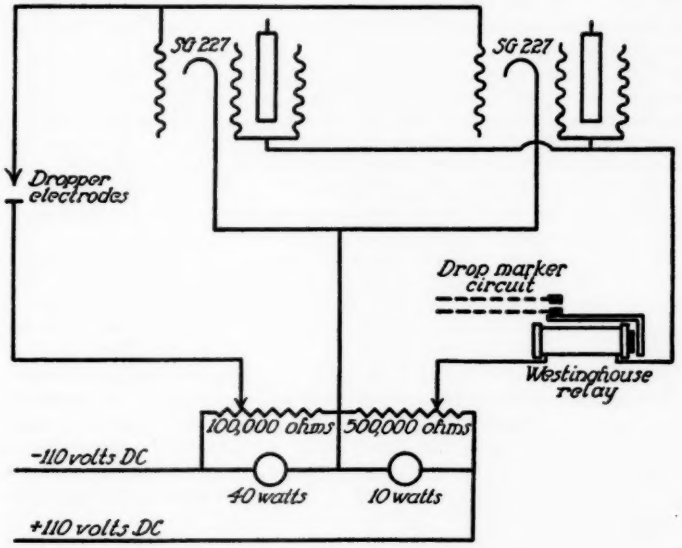

FIG. 4. CIRCUIT OF THE DROP-RECORDER

opening several centimeters above the orifice, and extends down the capillary tube to the outlet. The lower electrode consists of a platinum wire so bent as to hold a strip of paper-toweling approximately 2 mm. wide and 15 cm. long. The paper provides a surface film of water which snaps away each drop at the instant of contact; and its lower end dips into a wide basin of water. The size of drop varies according to the distance of the lower electrode from the orifice, a drop of 0.00125 cu. cm. being accurately counted. Rates of over 1000 drops per min. have been reliably recorded.

Master-control or robot. A robot takes the place of the experimenter in tripping switches that start and stop the tape-recording chronograph, administer stimuli and food. It will close or open a series of electrical circuits over a time-interval of approximately two hours. (The daily experimentation averages about one hour for each dog.) The switches can be adjusted so as to trip either

simultaneously or within 5 sec. of each other, or at any greater interval on up to the full 2 hr. span. The apparatus contains 72 switches at present, although the number of circuits that can be controlled may be increased indefinitely. The various stimuli noted above may all be administered by the robot. The tape-recording chronograph starts before each stimulus and stops after each secretory reaction. A permanent record is thus obtained of the points at which the stimulus began and ended, the point at which the food was given, and the number of drops of conditioned secretion (see Fig. 2).

The arrangement is as follows. Twelve arms are mounted one above the other on a shaft, and turn freely within 12 respective hoops of slightly greater diameter. The steel hoops are fixed to a supporting frame-work and do not turn. Small toggle switches are so mounted upon the hoops that they may be slid along the circumference and fastened at any point by means of a set-screw. The arms are driven by a motor, through a reducer, and turn in cumulative sequence. As the bottom arm turns about within its respective hoop it trips the switches at the periphery. Upon the completion of one revolution it catches the arm next above, carrying this second arm around with it during the second revolution. In succession each arm catches the one directly above, the last added arm tripping the switches in its hoop. At the twelfth revolution all arms are turning as one, the top arm alone tripping switches.

General procedure. Mechanization of the training involves certain basic principles which may be briefly considered. In Russian studies of conditioned reflexes, there has been a marked tendency to emphasize the individuality of each dog. Pavlov looks upon his animals as exhibiting definite 'temperaments.' Accordingly, the training procedure to be used with one dog differs from that most adequate for another. Furthermore, considerable flexibility is exercised in the routine employed with one and the same dog. For example, on one day 4 stimuli or trials may be given, the next day, 12; on one day a flashing light, a bell, and a metronome may be administered, the next day, 4 tactual stimuli. In some instances two new stimuli may be introduced on the same day. The interval between the beginning of the stimulus and the giving of food is continually varied.

It does not follow, of course, that because rats can be trained in a rigidly fixed routine, such a procedure is the most adequate for dogs. And while the great majority of studies in the animal field strive to keep the number and nature of stimuli uniform, Pavlov has offered considerable evidence to show that by the constant repetition of one stimulus a dog may be ruined for further work. In consideration of these two opposing points of view, the writer has followed the principle of using more than one stimulus during the day's experiment, but of administering the stimuli in a fixed order. For example, although a particular experiment is primarily concerned with the tactual analyzer, two other stimuli would be included in the daily sequence as follows, bell, bubbling sound, tactual: bell, bubbling sound, tactual: bell, bubbling sound, tactual. (The significance of position and time-interval may be partialed out experimentally when it is so desired.) In the above instance the bell and bubbling sound are added in order to give the variety considered necessary to prevent the dampening out of the reaction to the tactual stimulus.

Because the modern work in animal behavior has emphasized the variability of reaction—and there is abundant evidence that marked fluctuations are commonly observed in Pavlov's dogs—it seems imperative that an 'experiment' involving quantitative values should include sufficient measurements to permit an estimation of the range of normal variability. The writer does not advocate continuing a stimulus in the event that it becomes inhibitory, merely for the sake of uniformity, but he would emphasize the desirability of keeping the training-order fixed over relatively long periods in order to obtain a fair sample of chance variations in the dog's secretions.

SUMMARY

An automatic technique has been developed for establishing conditioned reflexes in dogs. A master-control device or robot administers the various stimuli and food, starts and stops a tape-chronograph which records the duration of the stimulus, the point at which food is given, and the drops of salivary secretion. Additional apparatus comprise the following pieces: Motor-driven circuit-closer, for timing intermittent stimuli; electromagnetic-pneumatic device for producing tactual stimuli; time-clock on house-current; electrically controlled food-box; and vacuum-tube drop-recorder.

AN INEXPENSIVE MEANS FOR TIMING OSCILLOGRAPH RECORDS

By WILMER T. BARTHOLOMEW, Peabody Conservatory of Music

In the preparation of apparatus for an extended series of oscillograph records, the need of a reliable timing device presented itself.[1] In oscillograph practice, when it is necessary to determine with precision the time-relations of phenomena to be recorded, it is customary to pass an A.C. current of known and constant frequency through an auxiliary vibrator, photographing the resulting time-line simultaneously alongside of the record of the phenomena. For frequencies in the musical range, however, the usual commercial currents would have been of no value, since in order to separate the sound waves sufficiently for proper analysis the film speed has to be increased so much that a low frequency of say 60 cycles if used for a time-line would give so broad a crest in the photographic record as to be unreadable with precision. This is particularly true in narrow-width films, where the most of the available space is reserved for the sound record itself, and the time-line, if of low frequency, must therefore be very 'flat.' Recourse is then usually had to a special generator delivering 500-cycle current, or to some sort of vacuum-tube oscillator. These latter are very satisfactory when good, but if they are as accurate as we like them to be, they are rather expensive.

Many investigators use the electrically-driven tuning-fork, but these are neither accurate nor reliable, in our experience. Contacts heat up, and require constant adjustment, sometimes at crucial moments. More serious, this changes the frequency characteristics. Even the microphone-driven fork is not entirely

[1]From the Department of Research of Peabody Conservatory of Music, Baltimore, Maryland.

free from these objections. Probably the most precise means known is the combination of a tuning fork and an oscillator in such manner that the fork controls the frequency of the oscillating circuit, and is itself maintained in vibration thereby. This, however, is expensive and complex, and a simpler method was developed as shown in Fig. 1:

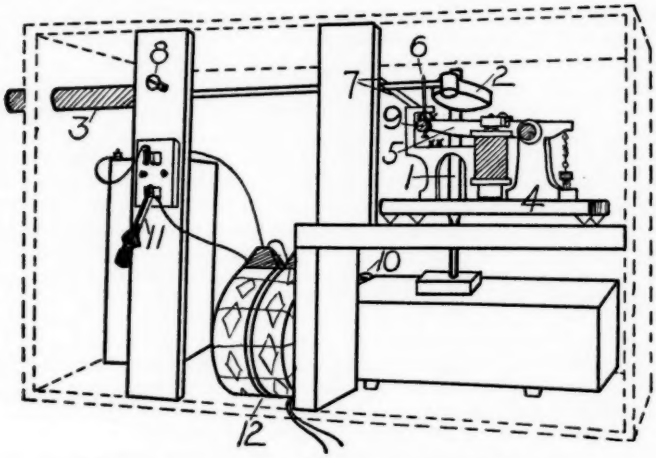

FIG. 1. APPARATUS FOR TIMING OSCILLOGRAPH RECORDS

A tuning fork (1) mounted on its resonator box, is placed so as to be struck by the felt hammer (2). The handle (3) of this hammer is rigidly secured in the wall of the box which contains the whole set-up. A telegraph sounder (4) has been modified and placed on a shelf so as to serve as a trigger to release this hammer. When the sounder-arm (5) is pushed down, the hammer can be bent back past the point of the nail (6). The sounder-arm can then be released, lifting the nail sufficiently to hold back the hammer-shank, which will bear against the nail because of the spring of the bamboo wood of which the shank is made. Obviously then, when the sounder-arm is depressed magnetically, the nail releases the hammer, which flies against the fork. To prevent movement of the hammer in the vertical plane, guides (7) have been provided. The 'orange-sticks' used by dentists proved satisfactory here, though any wood strip would do. Twine would doubtless be satisfactory here also. Wires were used at first, but proved unsatisfactory, because they were set into vibration themselves to a greater extent than the wood. A screw (8) bearing on the bamboo shank regulates the tension, and thus the strength of the tone. A set-screw (9), extending out from the sounder-arm, adjusts the height of the nail. Noise (which, if it reached the oscillograph microphone would invalidate the records) has been minimized by padding the striking points of the sounder-arm with felt, at the points marked X. In addition, the device (and in fact all of the recording appa-

ratus except the microphone, amplifiers, and control switches) is located in a dark room soundproofed from the studio where the singer or player performs.

The sound of the fork is picked up by an ordinary telephone transmitter (10) placed at the opening of the resonator-box, and concealed from view in the figure by the coils. The current from one dry cell, controlled by the knife switch (11), passes through this transmitter and through the primary of a small transformer (12). The transformer is necessary in order to keep D.C. out of the oscillograph vibrator. The secondary of this transformer is connected directly to the time-recording vibrator of the oscillograph. Various current strengths and transformer ratios may have to be tried before the proper amplitude be secured in the timing-wave. The vibrator used for this purpose in our set-up is a high-sensitive one, a product of the Westinghouse Company, requiring only 8 milliamperes per inch deflection on the film. Thus it is necessary to use only one dry cell, even when the tuning fork is struck fairly softly. It is advantageous to strike the fork lightly, as a greater constancy of vibration is secured. Our transformer was easily constructed by tying together two of the 'honeycomb' coils once popular in radio sets, although considerable trial-and-error was necessary to find the proper combination of sizes to give the best amplitude. A one-to-one ratio, using coils "L-200" or "L-300" was found to give best results.

FIG. 2. TYPE OF RECORD OBTAINED

This device can be operated in any situation where an electric impulse is available to trip the hammer. In the set-up in use at Peabody at the present time, a long belt of light material, with photographic recording paper fastened thereon, is driven past the oscillograph window. By means of a remote control switch in the studio, at the time when the record is to be made, a wire on this belt completes momentarily a circuit across two brushes sliding thereon, and this circuit by means of a relay lights the oscillograph lamp, starting the exposure. Another relay breaks the lamp circuit one belt-revolution later. By inserting the telegraph sounder that releases the tuning fork hammer in the circuit of relay No. 1, the time-line is started automatically whenever a record is started.

Fig. 2 illustrates the type of record obtainable. The complex wave here is of a violin tone, while the sinusoidal curve points off the particular frequency of the fork used. If a 500 dv. fork is used, the time-line will measure thousandths of a second from crest to trough.

The method has the following advantages. (1) Tuning forks vibrating freely at small amplitudes are extremely constant in frequency as compared with contact-driven or even microphone-driven forks. (2) The device is inexpensive. It was made entirely of odds and ends in our laboratory. Also, cost of operation is practically nil. Being simple, adjustments are few and easy. (3) Forks of any desired frequency may easily be substituted, giving any desired time-line. (4) The usefulness of the set-up is not restricted to the measuring of sound-wave records, for it can be used as a very accurate chronograph to measure the time-relations of any phenomena which can be recorded electrically —reaction-times, for example.

A DEMONSTRATIONAL COLOR MIXER

By G. L. FREEMAN, Northwestern University

There is one outstanding disadvantage in the use of rotating disks for color mixing; that is the confusion which often results when the student attempts to visualize the laws of mixture in terms of the color pyramid. A possible means of overcoming this would be to mix the colors directly upon a model of the pyramid. The following device indicates the feasibility of such a plan.

Fig. 1 shows an apparatus whose general form resembles the base of the color pyramid. The face of the 36 in. sq. frame is covered with black velvet, and the

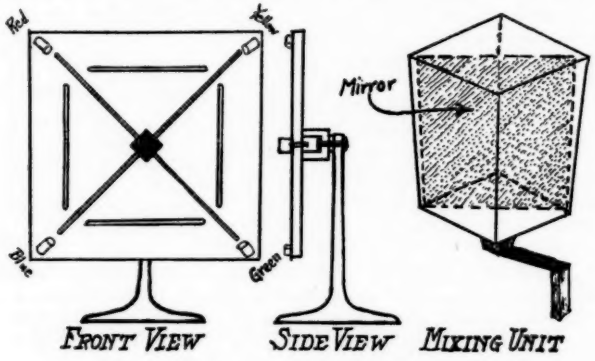

FIG. 1. A DEMONSTRATIONAL COLOR MIXER

four terminal or primary colors are painted on the tin reflectors in each corner. Each reflector carries a 100 watt electric light bulb with an appropriate yellow, green, red, and blue glass filter. A hinged cover in front of each filter makes it possible to shut off the illumination from any lamp without switching off the current. The frame is mounted in an upright position so as to face the audience. It can also be revolved about the axis at its center.

So far, the apparatus resembles the traditional frame for the demonstration of shadow contrast.[1] The innovation comes with the introduction of a mixing-unit. This consists of a square glass box with a frosted top (i.e. the plane exposed to the audience)[2] in which a mirror is placed at a slight angle so as to reflect the light outwards. The mixing-unit may be used at the center of the frame or at various other positions, as slits for the holder of the mixing-unit have been cut, as Fig. 1 shows, along the diagonals and parallel to the edges of the frame.

The following demonstrations may be made with this apparatus.

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[1]Cf. E. B. Titchener, *Experimental Psychology, Qualitative, Instructor's Manual*, 1901, 36; and E. C. Sanford, *Experimental Psychology*, 1894, 155.

[2]We used a square, frosted lamp with a ground glass plate fastened to the top.

(*1*) *Base of the color pyramid.* Take off the mixing unit and place a sheet of white cardboard over the entire frame. The lights reflected by this surface will fuse in a manner which suggests how the base of the pyramid would look.

(*2*) *Mixture of antagonistic colors.* (a) With the white cardboard removed and with the yellow and blue lights covered, move the mixing unit diagonally across the board between the green and red light-sources. Viewed from the front, the ground glass plate of the mixer will be seen gradually to lose in saturation of green, to become gray, and then to gain in saturation of red. The procedure may be repeated for yellow and blue light, using the other diagonal of the frame. It should be noted that in this demonstration it is necessary to keep the mirror of the mixing unit in line with the direction of movement: otherwise it will present an obstruction for the mixture of transmitted light. (b) Place the mixing unit at the center of the frame and leave all four lights uncovered. The ground glass plate appears gray. When each light is now covered in turn, the antagonistic color will show up in the mixing unit.

(*3*) *Intermediate color mixtures.* (a) Place the mixing unit in one of the slits parallel to the edge of the frame. Cover all lights except those adjacent to this line. Turn the mixing unit so that the plane mirror faces the sources of light. Move the mixing unit along the slit between the two lights, for example, blue and red. The audience will note the changes in that half of the ground glass plate which is illuminated by the reflected light from the mirror. The procedure may be repeated for other color-combinations. (b) Place the mixing unit in the center of the frame. Leave all lights uncovered. Keep the position of the mixing unit constant and slowly revolve the frame about it. The audience will note the changes in the color of the mixer as the various lights are reflected by the mirror. It may be explained that this demonstration is essentially the same as if one moved the mixing unit around the base of the color pyramid.

(*4*) *Simultaneous contrast.* This demonstration is much like that which is usually given with shadows. All lights are covered except one. The mixing unit is placed at the center of the frame so that the back of the mirror intercepts the light. This shadow is cast upon a piece of cardboard held behind the mixing unit. The audience will note the changes in the color of the shadow under different illuminations.

The pedagogical advantages of these demonstrations are obvious. With regard to their practicality, we can report that we have used the mixer with success even in partially darkened rooms. The cost of construction is minimal.[3] The chief difficulties come in the selection of appropriate filter glasses. A great deal of trial and error is necessary here in order to produce fairly well saturated light. We have found that a combination of colored glass and gelatin paper is satisfactory. For example, if the blue glass transmits a purplish light, use a sheet of green gelatin tissue to cancel the effect. Yellow usually presents the greatest problem and it may be found necessary to cut down the illumination of this lamp in order to equate intensities. Sliding rheostats might be introduced for all lights.

[3]We used tin coffee cans as reflectors.

A DEVICE FOR DEMONSTRATING APPARENT VISUAL MOTION

By Georgene H. Seward, Barnard College

The device described here provides a simple and inexpensive method of demonstrating the various types of apparent visual movement and the factors affecting them.[1] It is suitable for either individual or group observations, and is designed to form a permanent part of the laboratory equipment so that demon-

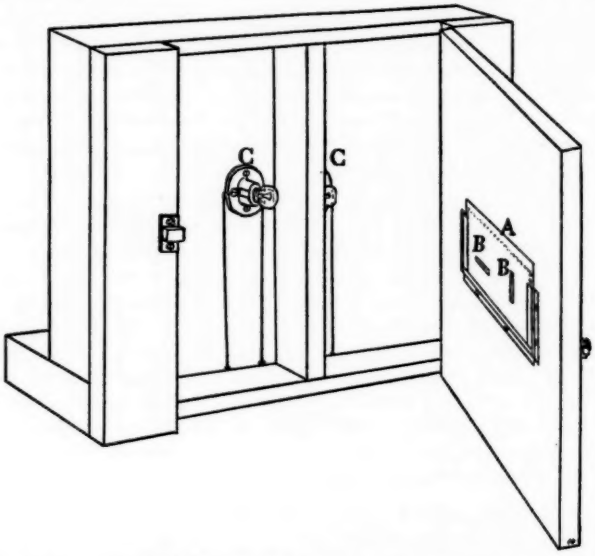

Fig. 1. Diagram of Demonstrational Apparatus for Apparent Visual Motion

Demonstration card (A) with two rectangular windows (B) is indicated in the card-holder. The lights (C), separated by a partition, are seen mounted upon a backboard. Wires are shown extending from the lamps through openings at the base of the backboard to connect with the batteries and metronome.

strations may be made by different experimenters and at different times under constant conditions. The apparatus consists of three parts: a large wooden screen, a light source, and a timing mechanism. The screen as is shown in Fig. 1, is hinged to side supports so as to operate like a door. In the center of this screen there is an aperture 9 by 18 cm. The inner side of this opening is fitted with a card-holder for the demonstration slides. Fig. 1 shows a slide (A), with two

[1]The writer wishes to acknowledge her indebtedness to Professor H. H. Keith of Hunter College and to Dr. J. P. Seward, Jr., of Columbia University, for helpful suggestions given her during the construction of this apparatus.

rectangular windows (B)—one horizontal and one perpendicular. Two 2.5 volt bulbs, (C), mounted upon a backboard directly behind the windows, constitute the light sources. Uniformity of the illumination of the stimulus-fields and the obscuring of outlines is attained by placing a piece of flashed opal glass behind the slide. The device is operated in the Barnard Laboratory by means of a mercury-cup metronome, but any timing apparatus which alternates the current to the two lights will suffice.

A partition between the bulbs prevents diffusion of light from one window to the other, while side and top boards prevent general diffusion. The outside of the apparatus is painted black. For the best effects the demonstrations should be made in a darkened room. Under these conditions 'movement' appears without the distraction involved in the actual movement of any part of the apparatus. This device, since it works automatically, eliminates the experimenter's reaction-time, and leaves him free to give whatever suggestions or explanations he finds necessary and desirable.

We may clearly demonstrate the various 'movement' types by using different slides. By varying the rate of the metronome, the effect of changing the temporal interval between successive flashes of light may be shown. Differences in intensity of illumination may be studied by increasing the voltage or using larger lamps. We may show the effect of hue by covering the flashed opal glass with colored gelatine filters. It is impossible with the present set-up, since the distance between the lights is fixed, to demonstrate the effects of spatial separation. This limitation, however, might easily be overcome by mounting the lamps upon small blocks of wood fitting into grooves along which they could be moved to coincide with windows placed at varying distances apart. If the above suggestions are followed this new apparatus will provide convincing laboratory demonstrations of the principal factors involved in apparent visual motion.

NOTES AND DISCUSSIONS

Insight

The term under discussion is one the common meaning of which is understood by all. "Sight into: thorough knowledge or skill: power of acute observation;" these are the definitions given in a small dictionary. But can psychology use such a term, with such meanings, in what it chooses to call a scientific context?

Köhler, who did use the word "insight" in his description of the mentality of apes, was drawing a sharp contrast between certain performances of these animals and the behavior reported by other investigators of animals in mazes and puzzle-boxes. Köhler found that his apes could discover a way of doing something quite suddenly and without a previous period of 'trial and error.' In a simple instance, an ape uses a stick for the first time to get a banana lying beyond its reach. Not only did the chimpanzee use the stick, which he then saw stretching in his line of vision towards the desired fruit, he used it appropriately, and he retained thereafter a special interest in sticks and other longish things in all similar situations.

This relationship between the stick and the fruit Köhler calls "a material inner, relation, . . . an interconnection based on the properties of these things themselves, not a 'frequent following each other' or 'recurring together.' "[1]

Köhler at least tried to give an unprejudiced description of the observable facts; but he went farther, and challenged us to account for this behavior strictly in terms of the principle of association.

In his more recent book—*Gestalt Psychology*—Köhler writes as follows:

> When I used this term in my treatment of intelligent behavior in apes, I ran the risk of an unfortunate misinterpretation. Since that behavior comprised extraordinarily conspicuous accomplishments which we did not expect to find in animals, insight would be readily misinterpreted as some special and supernatural faculty producing admirable and otherwise inexplicable results. As I used and intended the term, nothing of that sort should be implied in it. To avoid a similar misunderstanding this time, I have introduced the concept here in application to very common and simple facts.[2]

These "common and simple facts" are just those which distinguish "thorough knowledge" and "acute observation" from insufficient knowledge and vague or confused observation. But here many psychologists will object that "common and simple facts" lack a scientific status; indeed, that they are not facts at all, but meanings which, while they may serve well enough in a dictionary, cannot properly be counted as existential data either of consciousness or of behavior.

At this very point the Gestalt-theory parts company both with introspective existentialism and with a so-called objective behaviorism. I permit myself the use of the derogatory "so-called" in referring to behaviorism because, in my opinion, Gestalt-theory has more to offer at present to behaviorism than it has to Titchenerian existentialism.

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[1] W. Köhler, *The Mentality of Apes*, 1925, 219.

[2] *Op. cit.*, 1929, 371.

To consider Titchener's position first, I find nothing in it to which one can seriously object, if one is willing to accept Titchener's premises.

In the last analysis the validity of any set of premises is pragmatic. In Titchener's case the pragmatic validity of his postulates is to be found in his desire for a "pure" science of experience as such. I do not see how any fault can be found with a systematic attempt to study experience at its face value, or to classify it in accordance with its observable attributes or properties. Whether such an attempt will prove successful is another question—a question to which a negative answer has been given by all other schools of psychology.

A psychology of existential experience does not become a science simply because its theory is valid, but only when its data are acceptable as the pivotal facts of a system. It is well-known that these data are not acceptable to the behaviorist. If they were, he would have no just grounds for attacking their validity. "Logically," wrote Titchener, "a strict behaviorism can have no quarrel with an existential psychology, since there is no point of contact between the two disciplines."

Yet others beside behaviorists have attacked the validity of existential experience. Since Brentano's time the functionalists have insisted that the data of experience are but indications of the way in which minds work. The introspective observations of the Würzburg school, in their turn, revealed data which were not describable as existential contents; and finally the Gestalt-psychologists have shown that the attribution of uniformity to existential contents is untrustworthy, since it rests, not upon the actual contents of experience, but upon a figure-ground pattern which shifts markedly with the total behavior of the observer.

The insightless, meaningless, facts of existential psychology stand or fall with the possibility of demonstrating their existentiality. Logically one can make out a perfectly good case for their putative existence. But while logic can predict conscious contents, observation alone can demonstrate their existence as pivotal facts.

If we turn now to behaviorism, it is not so easy to say what this discipline can do with "insight, thorough knowledge or skill: power of acute observation." There are too many behaviorists to compare with the single Titchener, and their views obviously lack the consistency of a single highly gifted individual.[3] As for the views of the father of the movement, John B. Watson, we should agree, I think, that they have become a little warped by the method which this brilliant idealist has adopted in their presentation. I am also doubtful if Watson would seriously defend in private certain of his more striking public utterances.

If I must fall back upon my own notion of a behaviorist to answer my question what use he has for the concept of insight, I do so with the knowledge that no person is likely to step forward and identify himself with my picture. Since, however, my aim is to make real behaviorists of those who accept the name, I shall perhaps be able to escape the charge of setting up a straw man whom I can easily knock down.

My behaviorist, then, is as yet unaware of the use to which insight can and should be put in his system, and this, primarily, because he has not weaned himself away from the existentialism of the Titchenerian doctrine. My be-

[3]Cf. K. A. Williams, Five behaviorisms, this JOURNAL, 43, 1931, 337 f.

haviorist began his career by revolting against two things (1) the contents of consciousness and (2) the teleology of functionalism. In these respects he has much in common with Watson because the two systems of psychology with which Watson was best acquainted were those of Titchener and Angell.

My behaviorist has rid his doctrine of conscious states which can be described and ordered in terms of their own properties. Instead, he proposes to study what man and other animals do, and why they do it, in terms of their situations and responses. But although he has meant to reject the more important data of the Titchenerian system, he has only translated them to the biological level upon which Titchener, himself, made all conscious contents depend.

Titchener has described the major sciences in the following way:

> Psychology is the science of existential experience regarded as functionally or logically dependent upon the nervous system (or its biological equivalent); biology is the science of existential experience regarded as functionally or logically dependent upon the physical environment; and physics (including chemistry and physical chemistry) is the science of existential experience regarded as functionally or logically interdependent.[4]

My behaviorist rejects the first clause of this statement, and proceeds to differentiate his psychology from other biological disciplines by asserting for behaviorism a very special, if by no means unique, interest in the organism as a whole. While "existential experience regarded as functionally or logically dependent upon the nervous system" is superfluous to him, "existential experience regarded as functionally or logically dependent upon the physical environment" is his chief concern, The experience with which he deals is that of measurable changes or movements of the organism correlated with equally measurable changes or movements in the physical environment. Both the situations and responses are physical data, observed or experienced, and therefore quite as positivistic as Titchener's description requires them to be.

My behaviorist understands this epistemological implication. He knows that he is dealing only with the data of experience, mine or yours, recorded and measured by this or that instrument of precision. I am not sure that all behaviorists are so acute. Some of them talk as though they could observe data without the mediating agency of experience. But at least they all know, I take it, that they are not dealing with experience as an existential quality for itself. In this respect their revolt against the Titchenerian position is complete.

In confining himself to positive data gained through the medium of experience, my behaviorist is limited to the correlation of what he observes of the situation that confronts his experimental animal, and what he observes of the animal's response. Thus he limits himself unduly, and thereby destroys his concept of behavior as the total-response of the organism to its situation. He limits himself unduly because his fear of becoming introspective leads him to ignore what is taking place between the stimulation and the response. His method therefore, reduces itself to a kind of statistical correlation of situations and their prepotent stimuli, with responses and their specific muscular contractions and glandular secretions.

But behavior, both as the behaviorist would have it, and as the biologist finds it, is no statistical construct but a fundamental fact of organic life. With

[4] E. B. Titchener, *Systematic Psychology: Prolegomena*, 1929, 32 f.

respect to development, the creature's special organs are more a product of its behavior than a set of original parts which when assembled begin to behave. If we thus take behavior seriously, not as something to be explained in terms of something else, but as the fundamental premise of our science, it follows that "material inner relations" based on the properties of behavior itself will bridge the gap which otherwise appears to exist between situation and response.

This bridging the gap requires another description of behavior than that which has usually been employed in our science. In Köhler's words:

> It is the layman's belief that, in general, he himself directly feels *why* at one time he has one attitude, and later on another; also that, for the most part, he knows and understands directly *why* he is inclined to do one thing in a certain particular situation and *why* a definitely different thing under subsequent different conditions. In his view, then, he is experiencing directly and truly much of that dynamical context, the development of which constitutes mental life. Opposed to this belief and altogether foreign to it, we have the view of most learned psychologists at the present time. From their viewpoint, one is inclined to do one thing now and then another, because, in the first instance, certain nerve-paths are most available and, in the second instance, certain other paths are most open. Fortunate those people in whom the most permeable nerve-paths in practice are usually the right and appropriate ones! From the psychologist's point of view it would be a pure mystery if a person's behavior should ever be determined, as the layman believes, directly by the concrete properties of the actual situation. The layman's belief is in full agreement with everyday experience; the learned view harmonizes with what we suppose to be the viewpoint of natural science. I shall confess at once that I choose the layman's belief whole-heartedly.[5]

But in choosing the layman's belief it is necessary to do more than speak the word insight. How Köhler justifies the use of the term can be learned from the last chapter of his book, from which my quotation is taken. In our present context, I wish to consider the validity of the layman's belief that he knows, understands, in short has *insight* into what he is about.

No one doubts, I suppose, that there are intelligent and unintelligent forms of behavior, and that insight as the layman uses the term belongs to intelligence and not to stupidity. But we cannot say that insight is given whenever "things possess an inner material relation" . . . "based on the properties of these things themselves."

There are two points to the argument. The first is that "inner material relations" do exist in behavior, that they are of its very structure, and that they are not to be confused with a logical distinction of terms *and* relations, which would make the terms primary, and their relations secondary adjuncts. The premise we urge our behaviorist to adopt is that behavior is a going concern, a dynamic actuality, in which the organism incorporates its situation in its response, and likewise models its response in the likeness of its situation. In a word, the behaviorist should deal, not with situations and responses, but with *situation-responses*; for the one has no meaning without the other, whereas together they provide the fundamental meaning of everything the organism is and does.

The second point is that these material interconnections of the properties of behavior, while they constitute the grounds for intelligence and insight, do not reveal themselves as such until the organism is able to 'grasp' them. Now grasping requires one who grasps—one who knows and understands what he is about.

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[5] *Gestalt Psychology*, 1929, 349-50.

In the case of Köhler's ape insight is inferred from the manner in which the animal seizes the stick and reaches for the banana. It is not otherwise in man. But not all of man's behavior shows insight, any more than does the ape's. In neither case can the distinction be clearly made in terms of 'good' or 'bad' performance as determined by some objective standard of comparison. The criterion which the layman employs, and which Köhler so whole-heartedly accepts, is the way one 'feels' about it. This would seem to put us back into the realm of consciousness from which the behaviorist has escaped. However, it is not the existential experience of this feeling that matters, but the indications thus given of one's own or another's behavior. This use of experience as a means of determining the nature of something other than experience is in no wise different in principle from what is called 'objective' observation. It is not the feeling itself that makes insight, but the feeling that this concrete thing belongs to that concrete thing in the process of behavior. Feeling, in other words, is a mode of observation which reveals insight whenever the observed subject, be it myself or any other animal, behaves with an appropriate use of means to ends. We call such behavior 'insightful' because in our own case we 'see into' what we are about as we are about it. This means a complication of the pattern of behavior as compared with impulsive or chance combinations of means to ends. The complication involves a differentiation and a clarification of the course of behavior with reference to its beginning, its means, and its end. This differentiation is not prescribed by three things originally indifferent to each other—a hungry ape, a stick, and a banana; it is an achievement that may take place in a course of behavior which began with an impulse of desire for the banana. We do not know precisely when the ape begins to exercise insight in gaining what he wants. Neither do we know when we ourselves began to be intelligent. But we do know the difference between vague groping, with or without chance-success, and the phenomenon: "Eureka, I have found it!"

There must be a difference in these two kinds of behavior. 'Insight' names one of them. While it does not explain it to name it, it does state a problem which my behaviorist, who confines himself to situation and response as quite separate terms, cannot even state.

I have stated it for him in the hope that he will see the desirability of enlarging the scope of his inquiries by adopting as his premise a behavior which to begin with is an integral, self-regulating, process, to be studied in terms of its developmental aspects and properties, and not in terms of a pre-arranged framework of nerves which are excited by an entirely alien set of physical stimuli.

If I have made my points clear, it will be evident that all behavior is integral with reference to the formal course it takes. Its beginning, means, and end are formal aspects of a materially unified situation-response. Only by a differentiation of its aspects does the behavior warrant the attribution of insight which the dictionary calls "thorough knowledge and skill" or "power of acute observation;" because these attributes require distinctions on the part of the organism which clarify its behavior by setting the creature off from its tools and objects. Insight implies neither introspection nor telelogy, the two things against which behaviorism has revolted. But it does imply a difference between intelligent and unintelligent behavior, even though it draws no sharp line of differentiation between them.

With the postulate that behavior is always in some degree integrated, it is easy to find intelligence growing with a natural process of differentiation which provides new means of gaining ends. It is also easy to define the degree of insight by negative comparisons, where we can observe in others, or in ourselves (after the event), possibilities which are not, or were not, utilized. As Koffka says, "we call a behaviour stupid when a performance, all the conditions for which are given, is not carried out."[6] When the conditions are not all given, the differentiations requisite for insight are simply not made. But the resultant behavior, though it lacks insight, is not necessarily stupid. On the contrary, the behavior of lower forms of organisms, and the behavior of our own internal organs of digestion, circulation, respiration, and the like, are essentially 'wise' to the adjustments which they make in and with their respective situations.

Whether or not we choose to call this blind wisdom of behavior an evidence of intelligence is a matter of definition; but I should suppose that intelligence was somehow connected with the fundamental capacity of organisms to embrace situation-responses in meaningful ways.

Insight, however, is a term for noting an intelligent behavior in which the 'why' is registered by an appropriate discrimination of desire, means, and end: that is to say, appropriate in the only way in which any thing is 'realistically' or 'objectively' appropriate. For the use of the term 'insight' does not imply an idealistic philosophy, though it can be made consonant with such a view. Neither does it imply a purposive philosophy by its descriptive employment of such terms as means and ends. But it does imply that behavior is intrinsically meaningful, and that its data are neither the positive existential experiences which Titchener postulated, nor the independently existent parts of a machine.

It was my original intention to discuss a recent article by Mr. George W. Hartmann on *The Concept and Criteria of Insight.*[7] I have refrained from doing so in the body of my paper because Mr. Hartmann's concept and criteria seem to fall outside the context of my discussion. At the risk of an anticlimax, I shall make my comment on Mr. Hartmann's views in the form of an appendix.

Although Mr. Hartmann signs his article from the source and origin of Gestalt-Theory, the University of Berlin, I doubt if his statement of the case for and against insight would meet with the approval of the Berlin Gestalt-psychologists.

Mr. Hartmann's approach to the subject is that of definition and terminology. His "primary task," he tells us, "is to attempt, by means of a comparative and historical analysis, a reduction of the term to the level of a 'clear and simple idea.' "[8]

In the course of his article the author charges me with mixing badly the technical and colloquial uses of the term insight in my book: "Psychology and Education." I make no defense, because he is right. However, his insistence that insight can, or should, be used as a "clear and simple idea" strikes me as being questionable. Insight, as I have tried to show in the earlier part of this paper, states a problem rather than a solution. But the very statement of the problem rules out certain kinds of solution which Mr. Hartmann wishes us to test experimentally.

[6] Psychologie, in *Lehrbuch der Philosophie*, Max Dessoir, Ed., 1925, 528.
[7] *Op. cit.*, *Psychol. Rev.*, 38, 1931, 242 f.
[8] *Ibid.*, 242.

Mr. Hartmann's paper ends with ten "preliminary questions" which, he says, "must be settled before we can attain a satisfactory theory of insight."

I venture to quote and make reply to his questions without recourse to the "extensive program of research" which, he adds, "will be necessary before even the simplest of these problems can be answered."

Question 1: "Are specific insights the outcomes of prior trial-and-error?" *Answer:* In the context of Gestalt-theory there is no trial-and-error.

Question 2: "Does all learning require some insight, even of a rudimentary sort?" *Answer:* You must first define what you mean by learning.

Question 3: "What is the best single criterion (or group of criteria) of insight?" *Answer:* The observation that the behavior in question discriminates means and ends. I 'feel' my own discriminations, and I infer those of others.

Question 4: "Is insight necessarily accompanied by ideas? If so, what is their character?" *Answer:* In the context of Gestalt-theory ideas have no special place as a class of entities which can be said to accompany insight.

Question 5: "Is insight an active production of the organism, or does it arise spontaneously?" *Answer:* Both. The dilemma is not rigorous.

Question 6: "Is the moment of insight emotionally-toned? What is the nature of this coloring?" *Answer:* These questions imply introspective data which are irrelevant to a bare statement of insight as a special kind of behavior. The organism is undeniably 'moved' when it indicates insight; it may also be 'moved' without insight.

Question 7: "Can insight be identified with association by similarity?" *Answer:* Not in the context of Gestalt-theory.

Question 8: "Is insight a species of the genus intelligence, or vice versa?" *Answer:* It is a species of intelligent behavior.

Question 9: "How is insight related to inference and induction?" *Answer:* As types of behavior inference and induction are species of insight.

Question 10: "What is the connection between closure and insight?" *Answer:* All insight implies closure, but not all closure is effected with insight.

My answers are not all very significant, nor, perhaps, in every case sound, because Mr. Hartmann's questions are not framed as a Gestalt-psychologist would frame them. A functionalist or a positivist—either experiential or mechanistic—would probably answer these questions differently. The point is that such questions cannot properly be asked outside the framework of a definite set of postulates; and when they are asked the framework will suggest most of the answers as a necessary consequence of the postulates.

There is plenty of experimental research to be done before we can know all that insight implies, and whether or not we can give it the strict definition Mr. Hartmann desires, but I confess that my insight does not tell me how these ten questions can be translated into fruitful experiments.

Cornell University R. M. Ogden

The Method of Gestalt Psychology

"To understand Gestalt psychology one must understand its procedure, *how* its hypotheses are made, how they are translated into experiments which decide for or against them."[1] Thus speaks Koffka, one of the leading exponents of the

[1] K. Koffka. Some problems of space perception, *Psychologies of 1930*, 1930, 161-187. Further citations will be to this volume.

Gestalt theory. This statement itself reveals one of the outstanding characteristics of the method of Gestalt psychology. The hypothesis is made first and afterwards experiments are conducted to furnish the data. A sense of fairness demands that it be kept in mind that the evidence may be either for or against the hypothesis. Nevertheless, this method puts the user of it in the position of seeking proof, even when the evidence proves his hypothesis false.

The criticism of Koffka's method is not directed against a search for data to prove or disprove a theory, but rather against his use of an assumption, unsupported by adequate experimentation, as an hypothesis. A mere assumption in harmony with a general theory is not an hypothesis, no matter how convincing

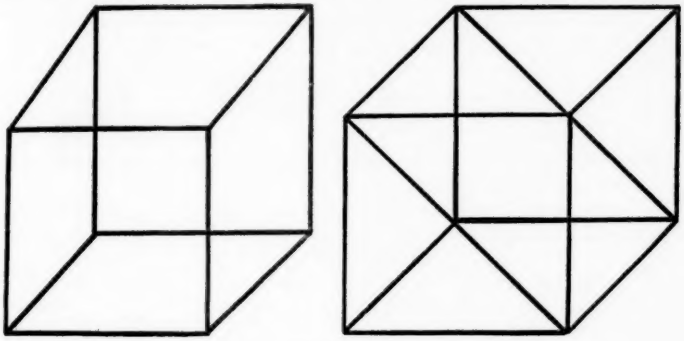

FIG. 1. KOFFKA'S FIGS. 1 AND 4
(a) designed to illustrate a solid, (b) a plane

may be the arguments in favor of it. Hypotheses are established not by argument and illustration, but by careful experimentation. The truth which advocates of Gestalt will eventually contribute to the science of psychology will have to survive in spite of their procedure. A casual reading of Köhler's discussion of "Some Tasks of Gestalt Psychology" will make this clear to the unprejudiced reader.[2] A better example of the fallacy of this principle is found in Koffka's treatment of "Some Problems of Space Perception."[3]

In his discussion, configuration or Gestalt is accepted by hypothesis. The writer then proceeds to prove the truth of the hypothesis by arguments based upon the visual perception of certain geometrical figures. The two figures on which the main argument depends are the Necker cube, (a) in Fig. 1, and a second figure, designated by Koffka as Fig. 4, (b) in Fig. 1, which is produced by adding two lines to the first figure.

Neither the statement of the hypothesis nor a detailed discussion of the argument is of significance with reference to the point at issue here, namely the method of procedure. Only so much of the argument as is necessary to make clear the fallacy of the method will be given.

[2]W. Köhler, *idem.*, 143-160.
[3]Koffka, *op. cit.*

Speaking of Fig. 1 (a), Koffka says: "This drawing appears to everyone as a cube, *i.e.* as a tridimensional shape notwithstanding the fact that in reality it possesses only two dimensions."[4] After discussing two other figures, omitted here, he continues: "Finally Figure 4, [our Fig. 1 (b)] which has been produced from Figure 1 by the addition of two lines, will again appear as bidimensional."... "None of these appearances is absolutely compulsory, but doubtless they are the spontaneous ones."[5] Koffka further argues that it is difficult to see Fig. 1 (b) in three dimensions.

Then follows an explanation and interpretation of these spontaneous perceptions in terms of Gestalt. In attacking the experience hypothesis as an explanation of these alleged spontaneous perceptions, Koffka says: "If the reader admits that Fig. 1 appears spontaneously as three-dimensional and Fig. 4 as two-dimensional, he would have to explain this by pointing out that Fig. 1 as a stimulus has more in common with a stimulus which in the past has aroused the experience of a cube than the second. Can such a statement be validated?"[6] This is followed by more argument in support of the organization hypothesis with more geometrical figures to support this argument. But to leave argument and to seek facts; what data are back of the assertion that "doubtless" Fig. 1 (a) is spontaneously perceived as three-dimensional, and Fig. 1 (b) as two-dimensional?

In order to find out how these figures are spontaneously perceived, the writer, with the aid of one of his students, carried on the investigation described below.[7] The printed pages on which the Figs. 1 and 4 appear in "The Psychologies of 1930" were covered with sheets of white paper. Holes were cut in these sheets so that each figure was plainly visible on a white background, as all printed matter and other geometrical figures were concealed. Beginning with the first grade these figures were shown to white children in every grade up through the senior high school. Then a sampling was made of negro children in the second, third, seventh and eighth grades. The figures were also shown to college students and to members of the college faculty.

Care was taken to show each figure first to half of the *Ss*. As a figure was shown, the experimenter asked the questions, "What is this a picture of? What does this look like?" Records were kept by grade, age, sex, and race; but the results showed that there were no significant differences due to any of these factors. In the lower grades, each figure was usually called a box. Occasionally Fig. 1 (b) was called a kite, but when asked, "What kind of a kite?" the answer was, "A box kite." In the upper grades the figures were generally called cubes, but frequently they were called boxes. One college freshman, more "spontaneous" then others, perhaps, called Fig. 1 (a) a "hexologan." When questioned further she said that it was a solid. Some called Fig. 1 (b) a diamond or cut stone. Care was taken to ascertain exactly what the terms used to describe the figures meant to those using them. "Diamond," for example, was not scored as tridimensional unless the subject gave unmistakable evidence that a solid was meant.

Table I shows how these figures were "spontaneously" perceived by the different groups.

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[4] *Ibid.*, 163. [5] *Ibid.*, 164 f. [6] *Ibid.*, 166.

[7] Miss Carol Jane Jones conducted the experiment in the public schools of Chickasha, Oklahoma.

TABLE I

"SPONTANEOUS" VISUAL PERCEPTION OF KOFFKA'S FIGS. 1 AND 4

Group	Grade	No. Tested	Perceived as: bidimensional Fig. 1	bidimensional Fig. 4	tridimensional Fig. 1	tridimensional Fig. 4
White:						
	1	38	1	3	37	35
	2	48	2	8	46	40
	3	49	0	5	49	44
	4	68	0	1	68	67
	5	73	0	1	73	72
	6	24	0	0	24	24
	7	28	0	2	28	26
	8	18	0	0	18	18
	9	26	0	0	26	26
	10	16	0	2	16	14
	11	16	0	1	16	15
	12	27	0	1	27	26
Negro:						
	2	10	0	0	10	10
	3	29	0	0	29	29
	7 and 8	60	1	8	59	52
College:						
Students		91	4	11	87	80
Faculty		40	1	3	39	37
Total		661	9	46	652	615

TABLE II

RECAPITULATION OF "SPONTANEOUS" VISUAL PERCEPTION OF KOFFKA'S FIGS. 1 AND 4

Group	No. Tested	Perceived as: bidimensional Fig. 1	bidimensional Fig. 4	tridimensional Fig. 1	tridimensional Fig. 4
White Children	431	3	24	428	407
Negro Children	99	1	8	98	91
College Group	131	5	14	126	117
Total	661	9	46	652	615

In the light of these results it appears that Koffka is approximately correct when he asserts that Fig. 1 is "spontaneously" perceived as a three-dimensional or solid figure. But the results are very much against his assumption that Fig. 4 is spontaneously perceived as a two-dimensional or plane figure. Only 46 out of 661 so perceived it. More than 13 times as many perceived Fig. 4 as a solid as perceived it as a plane figure. Yet Koffka's argument in support of the Gestalt hypothesis is based, in the article cited, upon the assumption that this figure is perceived as a plane surface.

It has been suggested that possibly the responses here recorded were not spontaneous because of the nature of the questions that were asked to evoke the response. It could be argued that the questions: "What is this a picture of? What does this look like?" are a stimulus which gives the *S*s a three-dimensional set. These questions were chosen after considerable thought, because they seemed to furnish a stimulus that would evoke an immediate and spontaneous response.

But to find out what effect mind-set may have on the visual perception of these figures a supplementary experiment was carried out with two groups of students in the summer session of the University of Oklahoma. In this experiment, Koffka's Figs. 1 and 4 were again used and also two other of his figures, Figs. 8 and 9.

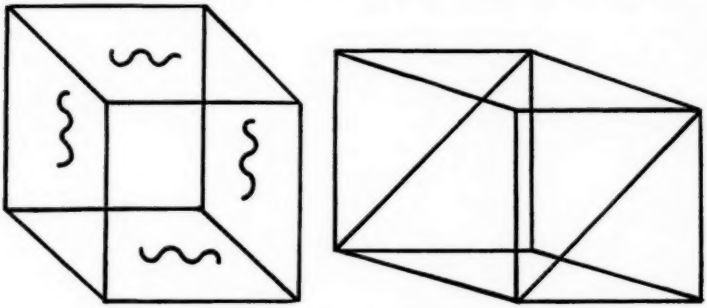

FIG. 2. KOFFKA'S FIGS. 8 AND 9

In the development of his argument in support of the Gestalt hypothesis, Koffka says that Fig. 8 is spontaneously perceived as a cube and Fig. 9 is perceived as a cube more readily than is his Fig. 4.[8]

These four figures were shown to the *S*s in the following order: 4, 1, 8, 9. This order was used to give the advantage both of mind-set and of first impression to Fig. 4, which is supposed to be perceived as a plane figure. Before showing the figures to an *S* he was told that the purpose of the experiment was to discover if he could see the figures as plane surfaces. Care was taken to avoid any mention of solids or three-dimensional figures. He was told to look for two-dimensional figures. The results of this experiment are given in Table III.

TABLE III

VISUAL PERCEPTION OF KOFFKA'S FIGS. 4, 1, 8, 9 WITH MIND-SET FOR TWO-DIMENSIONAL PERCEPTION

No. tested	Perceived as: bidimensional				tridimensional			
	Fig. 4	Fig. 1	Fig. 8	Fig. 9	Fig. 4	Fig. 1	Fig. 8	Fig. 9
57	4	4	3	6	53	53	54	51

[8]Koffka, *op. cit.*, 167.

Even with a mind-set for two-dimensional perception everyone of the four figures was perceived as three-dimensional by nearly all of the *Ss*. Fig. 4 was perceived as three-dimensional by 93% of the *Ss*; Fig. 1, by 93%; Fig. 8, by 94%; and Fig. 9, by 89%.

Koffka gives no evidence that he has devised any technique to eliminate the influence of past experience and of instruction on the perception of space. The term "spontaneous perception," therefore, is open to serious criticism, as used in his argument. No claim is made that the results recorded in Tables I and II are spontaneous, but only that they are as spontaneous as those assumed by Koffka. Any adequate theory of space perception will take into account the influence of training, both informal and formal, as a determining factor.

The purpose of this discussion is not to attack the Gestalt hypothesis of visual perception or to defend any other theory. But it is written to call attention to the fact that this procedure of Gestalt psychology may lead to conclusions that are not based upon facts. Or does it lead from conclusions? If the Gestalt concept is to make a permanent contribution to the science of psychology, there must be a change of method. Hypotheses must be more than assumptions, and experimentation must take the place of argument and illustration. The Gestalt concept has little to contribute to psychology if its other hypotheses are founded on no better evidence than that which Koffka presents to validate the organization theory of visual perception. Nevertheless, *Gestalttheorie* may be true. There is need for better technique and more experimentation.

Oklahoma College for Women HOWARD TAYLOR

FREE *VS.* MULTIPLE CHOICE ASSOCIATION AS METHODS OF STUDYING SEX DIFFERENCES

For the purposes of differentiating group interests, or "objects more frequently or readily thought about," one might reasonably expect free association to be more illuminative than controlled or multiple choice association.[1] Consider, for instance, a multiple choice exercise in which the subject is presented with a stimulus-word and several, say four, associated words, one of which he is directed to select; namely, the one that goes best with the stimulus-word, or the one of which the stimulus-word first makes him think. If, as frequently happens, some word or object other than those presented comes to mind, the *S* must either refuse to select a word, or select one at random. What is the good of asking the *S*, which of the following words, 'basement,' 'dark,' 'furnace,' 'vegetables,' goes best with 'cellar,' or of which he tends to think, when 'wine' is the word that comes first to his mind and seems to be the most fitting?[2] He mechanically underlines 'furnace,' and marvels at the naïvety of the examiner.

In free association, however, the *Ss* record their associations without let or hindrance, and genuine first associations are, theoretically at least, obtainable. As compared with a limited choice of associations, free associations may claim, (a) to represent the *S* more faithfully, and (b) to produce a greater variety of

[1]Part of a research in sex differences directed by Professors L. M. Terman and C. C. Miles of Stanford University. This study was made possible by a grant from the Committee for research in problems of sex, National Research Council.

[2]A free association score for high school *Ss* in a recent experiment was 21 males to 1 female associating 'wine' with 'cellar.'

associative material and be correspondingly more fertile of sex differences. The free method seems, therefore, to be, *a priori*, the better of the two procedures. Yet a comparison of a quadruple choice and a free association exercise recently administered with the object of disclosing sex differences disproved these assumptions. The free association exercise was found (a) to reveal sex differences less clearly than the multiple choice; and (b) to add nothing important to the findings of the multiple choice exercise.

A comparison of the responses associated with 12 stimulus-words chosen for this purpose at random from those common to a free and a controlled exercise recently administered to the same groups of *S*s suggested an explanation of this result, and exposed the disadvantages of the free association method. The comparison showed:

(*1*) That a majority, 54.5%, of the free responses fell within the same psychological class as the prescribed multiple choice words. By an associate of the same psychological class is meant an associate which, whether of the same meaning or not, clearly belongs to the same circle of ideas.

(*2*) That a change in one or more of the choice words could have materially increased this majority. For the four largest classes of free associates did not always agree with the four choice associates. With the stimulus word 'flesh,' for example, out of 391 free responses 192, approximately 50%, belonged to classes represented by the choice associates 'blood,' 'color,' 'meat,' 'soft.' But 121 free responses were 'skin' or 'limb' or words like them. If 'skin' had been substituted for 'soft' in the choice exercise the 50% would have risen to 90%.

(*3*) That the associates prescribed for choice included some members of a class (about 10%) not represented among the free associates, and yet yielded a significant though seldom a very marked sex difference. That is to say, whereas in free association only the stimulus-word can be used to reveal a particular class of sex difference, in multiple choice association both the stimulus- and the associate-words can be used for that purpose. This is a decisive advantage, because a defect of the free association method is that one cannot tell beforehand how many associates of psychological (sex-differentiating) significance will occur, or whether the yield will be large enough to be significant. Specifically, in free association economic, social, and religious words were far fewer than emotional words, words for parts of the body, or words for activity and adventure; and mechanical, scientific, historical and geographical words figured scarcely at all. This disproportion can be reduced, no doubt, by trial revision of the stimulus-words, but the simplest way to secure an even distribution of a representative variety of associate classes is to distribute the associates in advance as well as the stimulus-words. For example, the inclusion of 'enemy' as one of the four choices against 'face,' or of 'sin' against 'embrace,' induced sex differences over 'adventure' and 'religious' associates in neither case represented among the free associates with these stimulus-words. Sex distinctions can be obtained more adequately and with fewer stimulus-words by the multiple choice method than by the free association.

(*4*) That the free association method yielded certain sex differences precluded from the choice association in the nature of the case—in tendencies to associate synonyms, antonyms, and 'clang' words. Differences in these respects were not considered in our present inquiry, which was confined to meanings or contents; but if these 12 cases are a sample, free associates contain much more of

this material than is necessary to establish sex differences in these respects; and the excess is waste. Thus 827 synonym-antonym associates out of a total of 4050 available for all classes of sex difference is disproportionate. It may be added that waste of another class, namely, purely irrelevant associates (words that appear to have nothing whatever to do with the stimulus experience), occurred rarely in the present material, indeed not frequently enough to differentiate the sexes in the tendency to irrelevance itself.

(*5*) That the stimulus-word in the free association method tended to overdetermine the associations; it evoked so many of a particular class of associates (similar or cognate to the stimulus-word itself) that the other classes were reduced to insignificant and therefore non-significant numbers. The stimulus-word 'face,' for example, obtained 194 free associate words for parts of the body, out of 354 words in all. The quadruple choice ('enemy,' 'powder,' 'pretty,' 'watch,') assigned to 'face,' avoided words for parts of the body altogether. 'Danger' drew 264 (136/128)[3] traffic words, 'stop,' 'cop,' 'crossing,' 'curve,' 'flag,' et cetera, and 10 synonyms; which left only 61 for the multiple classes. A considered multiple choice can avoid such disproportions.

(*6*) That beyond the four prescribed classes there were with every stimulus-word some free associates belonging to other sex-distinguishing classes. With 'blush' went 'girls' (24/14) and words for parts of the body (10/16). But these extra responses with a given stimulus-word did not disclose items of sex difference beyond those covered by the control associates with other stimulus-words. Had they done so, this would have been due to the absence of words of these types from the choice of associates presented; consequently the remedy would be obvious. Not an inferiority of the controlled method as such, but a need of making the choices representative, is indicated. For this purpose free association provides one useful approach amongst others to the multiple selection, as suggesting 'promising' associate classes. Once a well-considered, evenly proportioned set of stimulus-words and choices has been developed, a regular use of the controlled association, with an occasional resort to the free, would seem to be the most practical and effective procedure in investigations of sex difference in association.

In seeking an economical treatment of free association material, we were led to the conclusion that the same sex distinctions would emerge from scrutinizing a sufficient number of sharply contrasting cases[4] as from sifting and sorting all the material. But we could not with free associations make sure of the contrasting cases for any particular psychological class being significantly numerous. For example, the contrasting color associates, 29/21, are too few to signify a male preference. But this is precisely the contingency which the multiple choice procedure can forestall. By selecting for its choices the most sex-contrasting words or word classes, and distributing them sufficiently and evenly, the numerical unevenness of the free results can be prevented; and we have instead a method of contrasting cases systematically applied.

[3]The number before the solidus (/) gives the number of responses by men or boys, and the number after it the number of responses by girls or women. In this instance (136/128) 136 men and 128 women gave traffic words in response to 'danger.'

[4]That is of the reponses recorded by a far higher percentage of one sex than of the other.

Differences in the character of the responses. Let us now consider the effects of the differences between the free and the controlled methods. In the controlled method the experimenter suggests the associations, in the free method they are left to the *S*s. The 'choice'—the presented associates—being limited and prescribed, the *S* concentrates more on comparing the values of the given associates than on giving association free play. The controlled method has no doubt less right to be called *association* than the free method, but it is not, therefore, an inferior instrument for deriving sex distinctions in interest. Put another way, in the free method the *S* reveals (or tends to reveal—he is liable to inhibitions) the word that he actually does associate; in the controlled method the word which, on however short consideration, he *decides* to associate with it as being in some way most relevant to the stimulus-word, without considering what he means by relevancy or by 'going best with it.' In the free method the *S* records any word that comes to mind whether relevant or not, for he is not judging relevancy. For the purpose of the study of sex-differences the controlled method seems to be the more valuable: it excludes mechanical and verbal associations, and it tends to reduce the influence of mere frequency. "These two words" (the stimulus-word and the word chosen) "are in my judgment members of a universe of discourse, or a circle of words, or word-referents, which go together better than do any other pairs" is what the *S* is declaring; and he is to that extent indicating a characteristic universe of thought, or sphere of interest, a topic to which one is emotionally inclined to attend, rather than a mechanical association by frequency or recency. But the free method abounds in mechanical associations—synonyms, antonyms, etc.—which do not correspond to interest in any vital sense.

Moreover, since the experimenter in the controlled method selects the associates, he is able to exclude the trivial ones and to direct attention to the valuable. This is scientific management, an economical use of available material.

In fine, while the free method supplies us contrasting cases in unpredictable proportion and at haphazard, it wastes energy on negligible differences and on trivial forms of association. The controlled method is better able to proportion the associate classes, and to confine itself to relevant cases that are indicative of genuine sex differences. The saving of time and trouble in assembling the raw material is also an additional argument in its favor.

Defense of the controlled method. We now turn to the criticisms against the controlled method. The first criticisms we met were that the *S* tended to record his choice at random, and the experimenter to overlook associative differences outside of the limited repertoire. These criticisms rest upon the asumption that the *S* exercises himself in free association, and that none of the choices given him would have occurred to him. Since these assumptions, as we have shown, are invalid, criticism lapses.

The second criticism seems more pertinent: that inasmuch as the associates are charged beforehand they do not disclose sex differences beyond them; and the more select the selection, that is the less it includes miscellaneous and random words, the more we restrict discovery. But, as has already been pointed out, this did not hold in the present investigation: the controlled method disclosed all the sex differences (except a difference in irrelevant associations) disclosed by the free method. The results of a separate experiment (not reported here)[5] with free

[5]Reported in a paper on "The use of free word association in disclosing sex difference" shortly to be published in this JOURNAL.

associations were confirmatory. Analysis yielded 13 items of noticeable sex difference. It was found that not one of these 13 went undetected by the controlled method, though the material for social, religious, and economic distinctions was, as with the free method also, somewhat meager. For these 13 topics the two methods proved very much on a par, while a fourteenth topic, scientific and mechanical preferences, was fairly adequately represented in the controlled method, but in the free method scarcely at all. By careful selection of associates all the ground opened up by the free method can, it seems, be covered by the the controlled.

In the controlled method we fix a number of routes, and keep to them. With the free method we wander here and there over several. In both methods important directions may escape us: in the controlled because we have left them out of our itinerary; in the free because they do not happen to come in. But the controlled method has the advantage that one can change the association routes as well as the starting points.

Summary. The responses to a quadruple-choice word-association exercise prove more economical, illuminating, and representative of differences between the sexes (and presumably between any groups compared) than free associations because (a) trivial, irrelevant, and random responses, prevalent in free association, can be avoided; and (b) a due representation of distinctive interests, which escape free association, can be assured beforehand.

The fact that *S*'s responses, in the controlled exercise, are relevant rather than mechanical increases rather than decreases its value.

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"PSYCHOLOGY TODAY:" A QUESTION OF ETHICS

Some years ago a moving picture entitled "Ingagi" occasioned considerable controversy over cinema ethics. In court the producer defended himself on the grounds that all moving pictures contained some "faking"—the house you see is merely a plaster façade, etc. Questions arose. How much of a picture *must* be genuine? What are the *ethics* of cinema production?

To the writer, Henry E. Garrett's essay, "Psychology today: A brief introduction for radio listeners," seems to raise a question of ethics. What psychologists present in technical journals stands as their own and is usually consulted by none but fellow psychologists. This holds to a degree for books as well. Even in popular magazines or newspapers the writings of a scientific man are still in large measure a matter which readers are free to value for themselves. An essay appearing under the authority of a "National Advisory Council on Radio in Education" is in a different category.

That no ultimately accurate résumé of modern psychology is possible does not excuse erroneous statements. An essay depicting "Psychology Today" must be at least up-to-date if for no other than *ethical* reasons. "Ethical," namely, because of the public for whom it is intended and because the name "National . . . etc." predisposes a relatively uncritical, relatively untrained public to accept the essay as gospel. The man into whose hands this booklet is placed is presumably uninformed regarding psychology today. This man cannot evaluate the introductory essay in terms of psychology today, else the entire plan, *which the writer heartily endorses*, would be idle. He cannot be uninfluenced by the words "National" and

"Education" in the Council's title. The question is really quite simple. How accurate must the introductory essay be if it is to play fair in this instance?

A few details. On p. 8 is the drawing of a cat in a puzzle box; we are told, on p. 9, that "the decrease in the time necessary in successive trials is a measure of the cat's ability to learn;" and, (regarding the learning curves on p. 10) that "improvement *nearly always* results from a gradual dropping away of false and useless movements, and from an equally gradual 'stamping in' of the right movements. Animal psychologists have found *little evidence* that animals in such situations 'reason' or 'think out' the correct solutions."[1] If the lay reader were familiar with Adams' work he would not be so dependent upon the statements of this essay.[2] It is because he is probably not that the writer feels a case of this sort at variance with fair play, a violation of trust and hence, in the long run, inimical to the science of psychology. The reader may not have consulted Adams' monograph but the author should have.

It does not suffice that the expressions "nearly always" or "little evidence" should be used. These quantitative terms are at best comprehensible only to those for whom the essay was not intended; at worst they are false for they suggest practical certainty ("nearly always"), which is not true, and practical insignificance ("little evidence"), which is incorrect. To assert that "animal psychologists have found little evidence" means *in this case* "no animal psychologist has found any appreciable evidence." Verbal niceties comprehensible to professional psychologists are wasted on the layman for *he* reads, "Psychology says animals do not reason." The statement is in consequence false despite its restrictive adjectives as printed.

Again, and this time without reserve, the author asserts (p. 13) that "observant or attentive application . . . [is] peculiarly a human accomplishment." Were the author unfamiliar with the literature of animal psychology this statement of a prevalent, if ill founded, popular belief might be excused. But the assumption seems not unwarranted that one writing of "Psychology Today" would have informed himself on these matters. It follows that the author either was not acquainted with the literature or that he has bid for popular applause by affirming prejudices of the untrained.

The case of animal psychology is typical and has been emphasized here as illustrating the point at issue. Fig. I, p. 7 indicates the usual conditioned response chart. That some learning occurs in this way no responsible psychologists deny. In a *technical* journal it is enough that one say, "The way in which fears . . . are *often* learned is illustrated in Figure I."[3] For *this* pamphlet the statement is misleading. The untrained reader sees a picture; *he* reads, "This is the way fears are learned." It is in the nature of things that the author's having qualified with "often" will probably pass unnoticed. In the competition: Figure I with its heavy lines and air of assured finality on the *one* side, the restrictive "often" on the *other*, the black arrows have every advantage. Hence whether he has printed it or not the author has in effect said to his reader, "*This* is the way one learns." (Compare also p. 16.)

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[1] The italics are mine. Discussion of these expressions follows in the text.

[2] Donald K. Adams, Experimental studies of adaptive behavior in cats, *Comp. Psychol. Monog.*, 6, 1929, (no. 27), 1-89.

[3] Italics mine.

We turn to the subject of emotions. If this field were as neatly organized as the discussion suggests, why have psychologists done so little toward a genuine ordering of our lives? Doctors, surgeons and others are integral to civilized life, why not psychologists? The answer is that when surgeons knew no more of human bodies than psychologists of human minds they were not yet more useful than are psychologists today. The study of surgery antedating its present skill is a matter of centuries. A medieval practitioner professing today's knowledge of anatomy would have been, to say the least, somewhat previous in his claims. Psychology does not perform in present society as does the physician because the science itself is not yet adequately developed. It is no disgrace, however, to be young. No one requires of a science that it reach maturity over night. To allege manhood when every act and feature betrays the youth is ridiculous. Psychologists are *constructing* a science: this is the essence of psychology today. Rash and premature claims do not lighten their task.

We read, p. 20, that "the management and control of one's emotional life is so comprehensive a topic that only a few hints can be given here as to how it can be done." There can be no other interpretation here that "it can be done = it is being done = psychology knows how to manage and control." That is what the sentence says to any but those intimately familiar with the details and fully aware that the illustrations of pp. 16-20 are at best merely indications of the direction in which certain psychologists are working.

As regards intelligence the same considerations apply. That much has been done in this field no one denies; that a tone of finality is not justified, perusal of controversial issues will quickly disclose. But those for whom the essay is intended are not at the time they read it in a position to undertake this perusal. There is no other conclusion than that the essay is unfair both to the reader and to psychology.[4]

Frankfurt am Main, Germany WILLIS D. ELLIS

CONCERNING THE ABSOLUTE METHOD IN PSYCHOPHYSICS

The present experiment is an attempt to determine the validity of a suggestion recently made by Fernberger.[1] When comparing the size of the intervals of uncertainty in a lifted-weight experiment in which a relative series followed an absolute series, it seemed that the intervals of uncertainty for the relative series were larger than one usually obtains.

In order to discover if this effect can be obtained with relative constancy and also if an opposite effect could be demonstrated if the relative series preceded the absolute series, experimental results were obtained from 24 naïve *O*s using the same conditions and instructions employed by Fernberger. The *O*s were divided into 2 groups of 12 each. For Group I the absolute series was given first and the relative series second—an exact repetition of Fernberger's experiment—and for Group II the order of presentation was reversed.

The results for Group I will be found in Table I. In the third column for the relative series it will be observed that there are only three values of the interval of uncertainty (I. of U.) smaller than 8 grm. while 6 values (half of the *O*s in this

[4]Attention is called to the fact that no reference is made to any other aspect of this booklet than the introduction.

[1]S. W. Fernberger, On absolute and relative judgments in lifted-weight experiments, this JOURNAL, 1931, 43, 560-578. Cf. esp. 573 f.

TABLE I

SHOWING RESULTS OF GROUP I

(Absolute Followed by Relative Series)

	Relative Series				Absolute Series			
O	h_1	h_2	I. of U.	P.S.E.	h_1	h_2	I. of U.	P.S.E.
1	.087	.082	7.12	97.40	.111	.077	9.28	91.49
2	.066	.061	9.46	92.99	.075	.085	4.84	89.06
3	.077	.074	7.53	96.42	.051	.063	13.13	92.37
4	.079	.082	9.44	95.15	.066	.072	7.46	91.09
5	.120	.131	8.32	94.78	.091	.088	3.97	89.27
6	.080	.088	10.00	96.55	.045	.062	21.65	87.08
7	.076	.114	9.98	98.49	.050	.049	13.28	85.88
8	.070	.086	8.78	95.31	.101	.162	1.28	95.55
9	.075	.079	8.15	93.48	.041	.052	16.48	93.79
10	.049	.121	11.15	96.79	.077	.119	9.84	92.28
11	.068	.076	7.11	96.47	.069	.228	8.66	94.11
12	.065	.079	13.23	97.62	.070	.059	6.52	91.56
Av.	.076	.089	9.19	95.95	.071	.093	9.70	91.13

group) give values greater than 8 grm. These results seem to confirm the observation made by Fernberger that the values of this interval are higher than one usually obtains for a relative series when it is preceded by training in an absolute series.

A consideration of the results for Group II (Table II) in which training was first given in the relative series strengthens this view. For these *O*s the average interval of uncertainty for the relative series is actually higher than for Group I:

TABLE II

SHOWING RESULTS OF GROUP II

(Relative Followed by Absolute Series)

	Relative Series				Absolute Series			
O	h_1	h_2	I. of U.	P.S.E.	h_1	h_2	I. of U.	P.S.E.
1	.083	.068	5.52	96.09	.093	.105	10.50	94.09
2	.087	.085	6.86	97.91	.093	.103	10.88	93.37
3	.086	.076	11.32	94.26	.079	.100	9.35	95.47
4	.074	.075	3.26	97.45	.052	.054	11.62	91.42
5	.060	.044	15.60	96.35	.074	.077	14.69	93.11
6	.053	.058	13.78	97.30	.077	.098	12.50	96.17
7	.074	.081	9.62	97.10	.071	.077	16.20	93.38
8	.093	.094	2.84	92.51	.087	.104	8.65	93.61
9	.115	.105	7.65	94.85	.092	.083	13.18	87.65
10	.059	.078	9.03	95.78	.096	.100	3.47	89.71
11	.096	.097	8.89	94.67	.092	.108	7.90	97.25
12	.038	.023	19.49	101.15	.109	.097	13.06	91.10
Av.	.076	.074	9.49	96.28	.085	.092	11.00	93.03

This higher average is misleading and is due to two extremely large values. But 5 of the 12 *Os* give values smaller than 8 grm. and the values for 4 of these are smaller than the smallest value for any *O* of Group I.

As a working hypothesis one might believe that this situation is due to the *Os* giving a relatively large number of 'intermediate' judgments in the absolute series and thus developing an attitude which is carried over and results in a relatively large number of 'equal' judgments in the subsequent relative series. If this be true one might expect a smaller number of 'intermediate' judgments in an absolute series if it were preceded by training in a relative series. An examination of the next to the last columns in the two tables would indicate that, if anything, the opposite relation is true; namely, the values of the intervals of uncertainty tend to be smaller in the absolute series if there is no previous training in a relative series. It would seem then that the larger values for the subsequent relative series obtained by Fernberger, as well as in the present experiment, might be accounted for in terms of the difficulty of making the adjustment from one series to the other.

It has seemed worth while to give all of the significant values for all *Os* in both series inasmuch as experience with the absolute series is as yet so limited. We may report that none of the 24 *Os* had any difficulty in formulating judgments in the absolute series. The statistical results confirm Fernberger's former study in several respects: (1) the values of the indexes of precision are of similar magnitude for the absolute and relative series; (2) the intervals of uncertainty tend to be larger for the absolute series; and (3) the point of subjective equality for the absolute series is lower than the central stimulus value, of a magnitude very similar to the effect of the error in the relative series.

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THE GENETIC PRIMACY OF HEDONIC TERMS

An interest in the genetic primacy of the different hedonic terms led the writer to make a study of the vocabularies of children. All the references which could be found to recorded vocabularies were consulted, in so far as they were available at the Harvard University libraries, and in all 61 vocabularies were tabulated. The group represented a wide range with respect to the intelligence and social position of the children. The particular words looked for in each of the vocabularies were *good, bad, like, love, pleasant,* and *unpleasant.*

Of the individual vocabularies there were only nine whose records were given chronologically for a number of years.[1] Their records are given in the accompanying table which shows the age in years at which the words first appeared.

An analysis of this table shows such little variation between children that we feel justified in making the following generalization. Of the six words studied,

[1] W. G. Bateman, *J. Educ. Psychol.*, 5, 1914, 307-320; *ibid.*, 6, 1915, 475-493; T. Beyer, *Educ. Rev.*, 49, 1915, 191-203; *ibid.*, 52, 1916, 478-489; W. Boyd, *Ped. Sem.*, 21, 1914, 95-124; M. C. and H. Gale, *Ped. Sem.*, 9, 1902, 422-435; C. W. Mickens, *Child Study Mo.*, 3, 1898, 203-205; N. M. Nice, *Ped. Sem.*, 22, 1915, 35-64; *ibid.*, 24, 1917, 204-243; *Proc. Okla. Acad. Sci.*, 4, 1924, 147-168; *ibid.*, 6, 1926, 317-333; P. M. Pelsma, *Ped. Sem.*, 17, 1910, 328-369.

good usually appears first, followed by *bad* and *like*, then *love*, and finally *pleasant* and *unpleasant*. While the material is not complete for the rest of the 61 vocabularies, examination of the data as a whole confirms such a generalization. In the supplementary data there were five more vocabularies in which the words *pleasant* or *unpleasant* appeared (in three cases at the age of 3 and one each at 4 and 6), but, by the only objective standard we have, that of extent of vocabulary, all the subjects using plesaant or unpleasant were precocious children. Particularly interesting is the fact that in none of the vocabularies up to the age of 6 years did the words 'pleasant*ness*' and 'unpleasant*ness*' appear.

	Cases								
	A	B	C	D	E	F	G	H	I
Good	1	—	2	2	2	2	—	3	1
Bad	3	2	2	3	3	3	2	2	—
Like	2	3	2	3	2	—	2	3	2
Love	3	4	3	4	2	2	3	3	2
Pleasant	—	—	—	—	—	—	3	—	—
Unpleasant	—	—	—	5	—	—	—	—	—

Since we seem to have discovered genetic differences in primacy of the common words used to cover the hedonic reaction, the question arises of their possible differences in meaning. An experimental investigation of the reasons for the differences in connotation of these words should prove stimulating for the field of affectivity.

Hanover, N. H. EDNA B. HUNT.

A REJOINDER

In a recent number of this JOURNAL,[1] Drs. Ferree and Rand discuss the difficulties they have met in my work on achromatic sensitivity.[2] Their difficulties are of their own making, and are due to an unnecessary insistence on geometrical optics. There is reason for believing that when all the dioptrical influences they suggest have been evaluated (as I have done) the anomalies will remain. My purpose was to show that distance, qua distance, was a factor, and that values determined for one distance were not applicable to another. This I have termed an anomaly from the point of view of geometrical optics.

One cause of confusion is their failure to restrict the term stimulus-distance to my meaning. In the first two pages of their criticism they argue that, as distance has remained constant in their work, its effect has been properly weighted. But this is weighting it only in the obvious and common sense of geometrical optics. It is not at all what I mean by the term, because in their view distance is treated only in so far as it affects size and intensity of retinal image. Later on however, they apparently meet me on common ground in the use of the term, taking distance as a variable which may influence sensitivity despite geometrical compensations.

[1] C. E. Ferree and G. Rand, Distance of projection of the visual image in relation to its apparent intensity, *op. cit.*, 43, 1931, 678-684.

[2] Ellis Freeman, The achromatic sensitivity of the dark-adapted retina in relation to stimulus-distance, this JOURNAL, 43, 1931, 246-253.

They imply that the anomaly is an artifact of uncontrolled dioptrical conditions (accommodation, pupil, etc.) which may account for inconsistencies. I cannot agree to this. It has seemed to me a matter of elementary precaution to anticipate these possibilities. Indeed I have elaborated this matter in tedious detail in earlier papers.[3] Earlier work shows the futility of dioptrical factors as an explanation of the anomaly, for as soon as one set of conditions is explained by this means, a contradiction arises from another set to which the identical explanation should apply. In one case a wide pupil and thin lens give lower acuity, a narrow pupil and thick lens higher (Aubert-Foerster phenomenon); and in another case the acuities for the same conditions are reversed.[4]

The published data of the work on achromatic sensitivity give internal evidence that pupillary variations cannot account for the anomaly, and that, therefore, an artificial control of the flux is unnecessary. Fig. 2 in my most recent paper[5] shows a drop at the fovea and a rise in the periphery with increased stimulus-distance. If the pupil were responsible, we should be obliged to attribute a drop and a rise in sensitivity to the same cause. Obviously this is contradictory. Drs. Ferree and Rand would have encountered no difficulty in eliminating the pupil as a factor had they not "purposely confined (their) comments . . . to central vision."

It is difficult to agree that "no special precautions have been taken to control fixation" at the center of the field. On the contrary, ample precautions were taken that do not suffer by comparison with the most careful usually employed. Weeks of preliminary training was given, and the subject understood the importance of reporting wavering. Still more important, fixation was physically easy, because each observation began with a stimulus considerably larger than threshold.

The tangent effect also is of no significance here. First it leads to the inconsistencies already described for dioptrical explanations in general. But there are two particular reasons why it is irrelevant. First, as determinations were made to only 20° of eccentricity, the effect would be but slight at best, and too small to account for the massive differences in sensitivity; and, secondly, if it did introduce a difference, the curves of Fig. 2 (mentioned above) ought to be horizontal, lying at different levels perhaps for each angle, but certainly not diagonal. Once a difference appeared, it should, since it is a function of angle alone, remain constant for all stimulus-distances along the same angle. Instead, as distance increases along any selected angle of eccentricity, sensitivity increases.

I must demur against their statement that my work gives acuity and not sensitivity. Logically their notion would prohibit the term sensitivity whenever stimulus-size was varied, and acuity whenever intensity was varied. Perhaps, if one wished, he could formulate sensitivity data in terms of acuity, even in a maximally simple situation as mine—a faintly illuminated disk in a dark room. But this would be inconsistent with what is understood by acuity, which implies

[3] Freeman, Anomalies of peripheral visual acuity, *J. Exper. Psychol.*, 12, 1929, 324-340; Anomalies of visual acuity in relation to intensity of illumination, this JOURNAL, 42, 1930, 287-294.

[4] Freeman, Untersuchungen über das indirekte Sehen, *Psychol. Forsch.*, 14, 1931, 332-365.

[5] *Op. cit.*, this JOURNAL, 43, 1931, 250.

the resolution of a detailed complex such as a letter. In my case there is only the differentiation of an illuminated mass from an unilluminated ground. If that were resolution, then any work on sensitivity would have to be called acuity, and it would make no difference if the units were ergs (Drs. Fernee and Rand) or mm. (mine). Perhaps what they mean to object to is the expression of sensitivity in spatial rather than energy terms. I see no objection to such an innovation, particularly when the uncertainty of energy measurements may be thus avoided.

I am in hearty accord with the suggestion that further work is necessary. That is why I have attempted no explanation of the phenomenon, and far from suggesting finality, I have deliberately chosen the term *anomaly* to emphasize its indeterminate status. While they expressly disclaim the intention to explain the anomaly, it seems that they have hazarded an explanation, though unsatisfactory, in emphasizing the dioptrical factors which they think I have overlooked.

University of Louisville ELLIS FREEMAN

PSYCHOLOGICAL LABORATORIES IN CHINA

Although two short notes concerning psychology in China have been published in this Journal,[1] no report has been made of the establishment and development of the psychological laboratories in China. Psychology, as a separate department of instruction in the universities of China, is not more than ten or fifteen years old; psychology, as science, is much younger; and psychology, as a laboratory science, is even more recent.

Laboratory courses in psychology were first introduced in China at Southeastern University in Nanking (now the National Central University), at the National Peking University in Peking (now Peiping), and at Fuh-Tan University in Shanghai. Fuh-Tan University seems to have been the first to have provided the Department of Psychology with a separate building for its offices and laboratories,[2] but this building is, as I am informed, no longer used exclusively by psychology.

Laboratories have recently been established at the Institute of Psychology of Sun Yat-Sen University in Canton,[3] at Yenching University and at the National Tsing Hua University in Peiping. The Academia Sinica (National Central Academy) of Nanking, which recently added Psychology to its field of activity, acquired a group of buildings in Peiping, October, 1930, that were completely remodelled to meet the needs of a modern psychological laboratory,[4] and active researches are now being conducted in the animal and neurological fields.

The Tsing Hua Laboratory. Although a Department of Education and Psychology had been in existence for some years at the National Tsing Hua University, Psychology did not achieve the distinction of an independent department until the autumn of 1928, when the joint department was divided and Psychology was given 5 rooms for its offices and laboratories. This was only

[1] S. K. Chou, Trends in Chinese psychological interest since 1922, *op. cit.*, 38, 1927, 487-488; The present status of psychology in China, *loc. cit.*, 664-666.

[2] Cf. Notes and news, *Psychol. Bull.*, 22, 1925, 140.

[3] *Op. cit.*, 24, 1937, 380.

[4] The National Research Institute of Psychology, *Academia Sinica and Its National Research Institutes*, 1931, 83-87. (The address of the Academia Sinica is 14 Ch'eng-Hsien St., Nanking.)

the beginning and entering wedge, for in February of 1931 the Department was moved to new quarters on the first floor of the newly constructed Biological Building which was partly endowed by the Rockefeller Foundation. Great credit is due to Professors Yueh Tang, K. S. Sun, and Ling Yeh for their efforts in behalf of the department. As the history of the development of the psychological laboratories the world over shows, psychology has had great difficulty in acquiring adequate space. It has very frequently had to be content with attic or basement rooms. Indeed, the Tsing Hua laboratory is located in the basement. It is, however, splendidly arranged and equipped and very few American and European laboratories are the equal of it. We feel proud of our laboratory

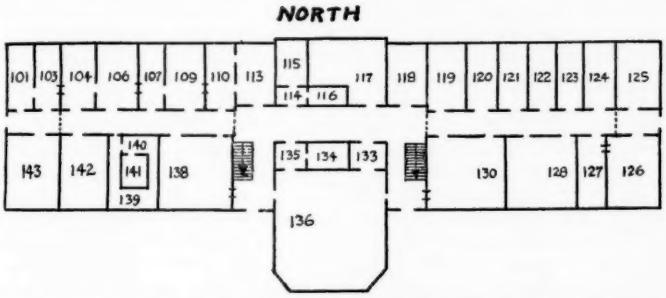

FIG. 1. FLOOR PLAN OF THE PSYCHOLOGICAL LABORATORY OF THE NATIONAL TSING HUA UNIVERSITY

and justified in claiming that it is the first one especially built for psychology in China, as well as being at the present time the most spacious one in China.

The plan of the first floor of the Tsing Hua Biological Building is shown in Fig. 1. A wide corridor extends throughout the length of the building and the large lecture room (used by all the departments in the University), office, research, laboratory, and seminary rooms, etc., open off of it. Two stairways at the sides of the large lecture room lead up to the second floor of the building. The front entrance of the building is on the north and it opens directly upon the second floor. Four other entrances, one at each side, and two in the rear, open upon the first floor.

Altogether 25 rooms are devoted exclusively to the use of Psychology—about half of the available floor space if the corridor, the large lecture hall (Room 136), the servant (115), boiler (117), and toilet (118) rooms are omitted. Eight rooms (101, 103, 104, 106, 107, 109, 110, and 113) are for offices or individual research; 5 are for the general laboratory (120-124); 2 for lectures (128, 130); 2 for dark rooms (116, 134); and 1 each for the seminary (119), animal laboratory (125), animal quarters (126), storeroom (127), apparatus (138), soundproof (141), library (142), and work shop (143). All the offices, research rooms, laboratories and the workshop have gas, a sink with running cold water, and D.C. electricity. The soundproof room, although not entirely successful in its construction, is perhaps the first attempted in China. The animal quarters is

heated by a separate heating system in order that the temperature of the room may be kept constant during the cold winter nights.

The apparatus in the Tsing Hua laboratory invoices at about 30,000 silver dollars, most of which has been purchased within the last few years. Some of the more expensive pieces were obtained from Stoelting and Zimmermann.

The library contains about 500 volumes and the current and back numbers of 30 magazines, of which 7 are complete.

Tsing Hua University SIEGEN K. CHOU

THE TWENTY-SEVENTH ANNUAL MEETING OF THE SOUTHERN SOCIETY FOR PHILOSOPHY AND PSYCHOLOGY

The Southern Society for Philosophy and Psychology held its twenty-seventh annual meeting jointly with Section I (Psychology) of the American Association for the Advancement of Science on Tuesday, Wednesday, and Thursday, December 29, 30, 31, 1931, at New Orleans, La. There were five sessions, all held in the auditorium of Hotel Monteleone, the headquarters of the Society, at which thirty-five papers were read.

The Tuesday morning session was devoted largely to learning and forgetting. Rex L. Hoke (Morhead State Teachers College) opened the program with a report on an experimental study of the effects of various factors on the accuracy of tossing steel balls 1-in. in diam. at a target 18 ft. away. The factors were (1) an electric shock automatically applied to *S*'s left hand when an error of a given distance was made, (2) information to *S* of his exact results after each trial, (3) direction of *S*'s attention to any phase of his results or procedure, (4) modifications of intervals between trials, and (5) simple instructions as to how the act should be performed. There were 11 *S*s, each having 60 trials. Punishment yielded fewest large errors, whereas knowledge of results effected the greatest number of bull's eye hits. Intervals of 6 sec. between trials were more effective than 2- or 4-sec. intervals. Direction of attention to the painful consequences of errors increased efficiency as did also putting emphasis on success. Certain aspects of the results indicate the need of using many subjects of different age, sex, etc. in further experimentation.

Richard W. Husband (University of Wisconsin) reported a study of sex differences between a group of 40 college and 20 high school women, on the one hand, and 30 college and 17 high school men, on the other, all of whom were tested on the following five parts of R. H. Seashore's motor skill battery: Serial Discrimeter, Pursuit Rotor, Spool Packing, Crank Drill, and Tapping. There seemed to be slight and uncertain sex differences, except in one of the last two (simple) tests. Comparisons of this sort certainly need very careful control of such attitudes or sets as are affected by different social factors.

Goodrich C. White (Emory University) found in studies on memorizing that the *S*s with poorest initial ability improved most with practice, though there was generally a slight decrease in efficiency in continued practice, a slight advantage in retention of content near the end of the period of continuous memorizing (attributed by the author to 'retroactive inhibition'), and great similarity of learning curves under varying conditions. A striking feature in the curves seemed to indicate a 'final spurt.' This most certainly indicates, however, some sort of unevenness in stimulating conditions.

O. A. Simley (University of Arizona), assuming that the strength of association bonds between a series of geometrical characters and their respective nonsense syllables is measurable in terms of the speed with which they function, reported a study of relations of sub- to supraliminal learning. He had his *S*s associate 360 meaningless geometrical figures with nonsense syllables in pairs of 9 in each of 40 series. Serial orders were varied to compel only cross (figure-syllable) associations and supraliminal associations were measured by the reaction time between the appearance of the figure in Hull's exposure apparatus and *S*'s verbal response, while subliminal learning was "measured by the number of promptings required to establish the bonds." Learning was (as may be expected in the case of such materials and method) more rapid in the initial stages of the process, and there was a positive correlation, apparently high, between sub- and supraliminal learning both as to quickness of response and as to the slope of the rising curves below and above the threshold. It was hardly obvious from the report that the two different sorts of learning situations were near enough identical or controlled for direct comparison of results.

Fred McKinney (University of Missouri) had had 93 college students learn twelve lines of one poem and 95 learn an equal amount of a similar poem, five minutes being devoted to learning and five to recall. These students were then divided into three groups of about 60 in each; one group learned a second equal amount of poetry, another a list of nonsense syllables, and the third group listened to a Seashore discrimination record. All the students finally recalled what they could of the original twelve lines of poetry. This final retention test was repeated again a week later. No interference ("retroactive inhibition") appeared in the final recall, but only in that a week later. The conclusion that "poetry is not susceptible to retroactive inhibition upon immediate recall" is certainly subject to several conditions which need careful study before such theoretical implications as were inferred in the paper can be safely indicated for a "theory of retroactive inhibition" in general.

John A. McGeoch (University of Missouri), in a discussion of the concept of forgetting, pointed out the need of some systematic attempt to determine the boundaries of "forgetting," which is only the partial obverse of retention. From examinations of phenomena usually classified as forgetting he found only one that stands out as characteristic of all cases—"a loss in the functioning of a learned activity." He concluded also that there is no logical ground for a sharp distinction between this sort of loss or decrement and that resulting from continuous work or from other special conditions.

H. H. Remmers (Purdue University) described experiments which have been in progress for four years on students in beginning courses in Spanish, German, and French, who were grouped according to ability as measured by freshman entrance placement tests and according to certain other factors, and who were graded on test questions according to a standard procedure. No results are available to the reviewer.

The afternoon session, devoted in the main to sensory and perceptual phenomena, began with an informal report by P. F. Finner (Florida State College for Women) on attempts at measuring the differential intensity limen between the several pairs of primary colors and between each color and white. Wave-lengths were kept constant by means of color filters and intensities were controlled by the photo-electric cell. Forty-six *S*s, apparently college students, were used. In-

dividual differences in brightness thresholds were large, and the correlations between thresholds were highest in the cases of colors near together in the spectrum (*e.g.* R and Y, 0.72; B and G, 0.92) and were low between thresholds of complementary colors (*e.g.* R and G, 0.06; Y and B, 0.08). In the discussion it was developed that exposure times were not controlled; each *S* observed till he saw the difference. This time-of-stimulus factor, it was suggested by H. M. Johnson, was not controlled in Weber's law determinations though the limen is obviously a function of the duration of stimulation. Geldard suggested that correlation differences noted in the paper were due to absorptions of the photo-electric cell which was used practically as a photometer.

W. F. Smith (University of South Carolina) reported an experiment on the relative quickness of visual and auditory perception. Using four different intensities of the sound and also of the light stimuli with either simultaneous or slightly divergent presentations (controlled by means of contact attachments on both ends of the 'hand' on the face of the Hopkins chronoscope, and two movable contact points on the edge of the face), he was able to vary according to predetermined schedules the relative times and the intensities of the two stimuli. One hundred forty-four possible intensity and interval combinations were presented individually to two groups of college students, 40 in each group. Those of one group were to report at every trial which stimulus came first; and those of the other group, whether there was an interval perceived between the stimuli. No doubtful reports were permitted. Results show that by a suitable variation in intensity either the visual or the auditory stimulus, when presented at or near simultaneity, can be made to appear first in time. The uncertainty range can therefore be affected by an increase in the intensity of either the visual or the auditory stimulus. The possibility of an absolute speed-of-perception measurement of visual and auditory stimuli based on the rheobase in chronaxy is suggested.

Lyle H. Lanier (Vanderbilt University) gave a report of progress on his experiment in cutaneous denervation. The general interrelations of the several "modes" of sensation, both immediately following denervation and during regeneration of the nerves, were emphasized. The areas of anaesthesia to warmth, cold, light touch, and pain stimuli at three-week intervals were shown in lantern slides. These were all from only one of the three *O*s (Mr. Lanier) serving in the experiment. Dissociations occurring both immediately after denervation and during regeneration indicate the existence of four separate afferent mechanisms corresponding to cold, warm, pain and contact, and support the quantitative theory of Nafe; but these four forms of sensitivity, however, tend to form two general groups—a thermal and a contact-pain group. The virtual coincidence, in general, of anaesthesia areas for contact and pain and for cold and warm (a much larger but closely corresponding area), suggests interesting neurological problems. Head's theory finds little support in these results. The 'intermediate' zone does not uniformly show the protopathic complex of attributes; the differential order of return, required by his theory, is not found; and "the protopathic aspects of sensation—hypersensitivity and 'reference'—occur on skin responding to light touch, showing apparently no 'inhibition' of the former by the latter."

Max F. Meyer (University of Missouri) called attention to a recent 'refutation' by Galperin of the old theory that the Poggendorff illusion is due to the overestimation of small and the underestimation of large angles. Not denying the effect of such wrong estimations of angles, Meyer pointed out that ten years ago (*Psy-*

chology of the Other-One, 1922, 231-238) he presented a refutation based on a simpler and a more convincing principle. It will be noted, however, that Meyer's explanation is based on double and triple oblique lines (p. 233) and that a single oblique line interrupted by a vertical plane figure, say, shows the phenomenon as effectively as two or three such lines, even though Meyer's explanation does not, it would appear, apply to such a case. The problem treated theoretically, as to the relative effects of different factors (whether innate or acquired) seems to be one that can be solved experimentally by testing different hypotheses on the basis of the judgments, under controlled conditions, of a large number of unsophisticated *Ss* at different ages.

C. C. Ross (University of Kentucky) reported some apparently preliminary experiments on the reliability of students' ratings of different subject matter according to specified characteristics. For example, the ratings of instructors, "according to a certain rating scale," by approximately 200 students ranging from freshmen to and including seniors, and a re-rating "without warning a week later" varied from slightly negative correlations to 1.00, "the central tendency usually being in the 0.60's and 0.70's." He concluded "that many of the current studies employing student ratings are to be taken with the traditional 'grain of salt'." That is, of course, not a new discovery; moreover, Mr. Ross had seemingly overlooked the precautions against memory and other effects of earlier ratings, as well as the fact that both of the ratings of instructors may be influenced in the same way by rather well known 'reputations' current about the campus.

G. C. Brandenberg (Purdue University) reported results of two investigations, two years apart, into the conditions "which might be classed as obstacles to the best quality of instructional work in the class room." He was chairman of a committee organized to discover and remove such obstacles. A questionary was sent to the instructional staff at the time of each investigation. The five most serious obstacles were (1) the large amount of time required for reading students' papers; (2) bad light, heat, ventilation, etc; (3) indifferent attitude of students (evidently only a *result* of other conditions); (4) heavy teaching loads; and (5) unsatisfactory office conditions. A close agreement was found among all the ranks of the instructional staff but departmental heads and assistants, indicating, probably, that "the individual who has the most to do with teaching conditions—does not see teaching problems in the same way as do teaching members of the staff." Considerable improvement in these conditions was found in the second investigation.

Joseph Peterson (George Peabody College) reported data showing a serious deficiency throughout the United States in the training of psychology teachers in state teachers colleges and normal schools, as compared with state colleges for women, standard colleges and universities, and the better privately endowed institutions. From the two reports on the qualifications of institutions and teachers of Psychology in the South, issued in May, 1928 and 1931 by the Committee on the Teaching of Psychology (of which he was chairman during its first five years of work), it was shown that the increase in properly qualified teachers—those holding Ph.D. degrees with major work in psychology—during the three-year period indicated was 54%. In the corresponding period the increase in members and associates of the American Psychological Association was 37% in the other 32 states, and 40% in the entire United States. This rapid improvement in the South he implied was an indication of the effectiveness of the Society's work through its committee. The number of qualified psychology teachers per million

population in the year 1930-1931 in each state was found to be as follows: Alabama 3.4, Arkansas 1.1, District of Columbia 14.3, Florida 4.1, Georgia 2.8, Kentucky 4.2, Louisiana 2.4, Maryland 12.2, Mississippi 1.0, Missouri 4.1, North Carolina 4.1, Oklahoma 2.5, South Carolina 1.7, Tennessee 5.9, Texas 2.1, Virginia 5.4, and West Virginia 4.7; all combined, 6.1. There were at the same time 9.5 members and associates of the American Psychological Association living in the other 32 states and 9.1 in the entire United States per million population. Since a considerable number of the associates have not yet earned their Ph.D. degrees in psychology, the difference between the South and the other sections in number of psychologists per million population is not as great as these numbers suggest, and the comparative gain in the number of psychologists is even greater in the South in the triennium indicated than the percentages show.

The Wednesday morning papers were largely theoretical and philosophical. A. S. Edwards (University of Georgia) began the program with a discussion of "The Concept of Normality." He contended that psychological problems are "similar to those in the other sciences, and, as in those sciences, need to be based upon averages." He also raised problems, confronting one in the handling of groups, as to the size "of the normal group, the amount of deviation from the average that can be admitted to the normal group, etc." There seems to be nothing new in the paper.

In the absence of Knight Dunlap (Johns Hopkins University), his paper on emotions as teleological constructs, was read by Buford Johnson. Dunlap points out that while the 'emotions' were definitely and justifiably aligned with the 'instincts' by William James, the text books continue to treat the 'emotions' in the old fashion even though they have abandoned 'instincts,' and that the behaviorist "bases his theory of emotional development on discrete primary 'emotions'." He contends that emotions are clearly artifacts "determined by reference to the situation in which the reactor is placed, and by the interests and purposes of the classifier." "Any of the conventional names may apply to widely different affective conditions, according to the interest of the namer; and the same affective state may have diverse names according to this interest." Reports by individuals, that they feel depressed, elated, etc., are only the application of previous ideas to their experiences. The errors resulting from a rough application of such views are obviously obstructive to research on the emotions. One finds similar difficulties, of course, in any sort of classification or description, or procedure by verbal methods or symbols.

Herbert C. Sanborn (Vanderbilt University) traced briefly the historical developments in aesthetics, and pointed out that there has been a persistent attempt to build an objective science of art. However, this science, according to certain writers, is to exclude everything psychological, their reason being chiefly that psychological aesthetics fails to do justice to the field of art. Sanborn held that disputes of this sort are ultimately metaphysical in nature, and that while investigations should doubtless be carried on by every method available, the question "whether there is any one method which embodies all the others" should be left to metaphysical revision. Specific interpretations of aesthetic experience are valid in so far as they are in harmony with the systems of which they are specifications, while the validity of these in turn would reside in their inclusiveness and coherence, that is, the degree to which they realize absolute system. Concrete illustrations were given. There is certainly ground here for inquiry into what ab-

solute aesthetic systems would mean and how they could be arrived at if not through the experiences of individuals.

Harold N. Lee (Newcomb College, Tulane University), discussing metaphysics and aesthetics, contended that the usual procedure of approaching aesthetics from the viewpoint of the metaphysical doctrine, is exactly the reverse of what an application of the scientific method would indicate. Aesthetics, which deals with one of our fields of experience, "must enunciate its principles independently of any formulated metaphysical doctrine." From the metaphysical point of view, he said, any set of aesthetic principles would be subject to the interpretation indicated by the metaphysical theory of the interpreter. In the discussion E. S. Edwards objected that psychology did or should start with particular experiences, and Sanborn held that science is today beginning from the most sweeping categories. He referred to the work of Percy W. Bridges as an example. Lee replied that the scientific method does not deny analyses and that he said nothing to identify science with induction, except the natural sciences.

Morten ten Hoor (Tulane University), in an examination of introspection, said that "in all philosophical disciplines generally, the concept of introspection has always been accepted as an ultimate." Introspection, however, seems to have had but little analysis by the great philosophers and psychologists of history. The traditional double-self notion is untenable because it implies a dichotomy of experience, something more than immediate experience. The notion of consciousness *per se* turns out on analysis to be an abstraction, a realistic (medieval) universal. But there are only types of awareness distinguished by types of sensation localized within the body, on its surface, or without it (as the redness in a rose). Usually this awareness has been assumed to be within the body (hence introspection), but it is where it is experienced. Any awareness content can, only within certain limits, be verbally reported. What realities underly awareness is a question of inference from immediate experience. Certainly no metaphysical eye or self is given in immediate experience; hence the error of claims making introspection the source of knowledge of the self or of experience. As a criticism of radical behaviorism, it was suggested that "behavior can enter experience only as sensory awareness;" that pure behavior or behavior as such is as much a metaphysical assumption as the ego or self." A layman in philosophy might suggest the need of inquiry into the concept of "experience." Is *experience* identical with *reality*, or what is the relationship here, and what the evidences? Furthermore, are 'feelings' located?

W. H. Walker (Berea College) pointed out that the concept of causality had arisen rather by implication than by any direct observation. "The patent fact is that we do think of the causal relation as a necessary one when we apply it in the physical world." But this assumption is justified in the large only in a general way. "We never have observed the same conditions producing the same identical results." (How could this be possible, considering temporal aspects?) He regarded it as improbable, "that the present repudiation of uniformity in the action of electrons and quanta will be permanently accepted. We cannot think of nature in terms of caprice." As "this is a matter of the constitution of our thought, called into play first by the suggestions of experience," so is also our thinking of ourselves as "creative causes, at least in some of our decisions. This concept of creative personality has precisely the same ground and the same validity as the concept of mechanical causation." This is a rather unique 'logical jump' to make on such

grounds! Why did man leave his primitive thought of nature as animistic, and, in modern times, when better means of observing (under experimentally controlled conditions) are possible, find the *vera causa* view untenable even as applied to himself?

Charles A. S. Dwight (Keuka College), closing the program with a paper on "The Shadowland of Science," indicated changes that have occurred under the developments of modern physics: the crude lump-matter theory of the 19th century has gone and the "oppression" of the old coarse materialism has been relieved; physicists now tend toward becoming quasi-metaphysical, and mechanism [the *bête noire* of the theologian] is wanting as an ultimate explanation; and "the principle of physical indeterminism breaks into the old 'closed circuit' idea of nature with something that looks like a mandate for freedom." He therefore sees new possibilities for consciousness and will "as causal factors" and for "faith or immortal hopes." "The spiritual view of the world is, if anything, more easy or likely than before. Science lingers among the shadows, but faith walks among the candlesticks."

Does this premature outburst of hope based on old conceptions of will and freedom not seem to reveal a sinister motivation that is below that of philosophy as a search for truth? Is this the way out for those whose conception of freedom has failed, or may they also not hope and seek for new conceptions?

The afternoon session was largely devoted to child development and measurements. S. A. Courtis (University of Michigan) reported that data from 30,000 school children in America, England, Italy, Scotland, and Switzerland measured as to height, weight, broad jump, eruption of teeth, and tested with two intelligence tests and one educational achievement test, revealed on analysis the influence of three sets of specific factors—starting points, environmental influences, and maxima at maturity—and one general factor—rate of growth. He found in these conclusions the possibility of "a new point of attack upon the analysis and interpretation of psychological measurements, and upon the prediction of growth."

John E. Anderson (University of Minnesota) reported result of an analysis of data obtained in connection with a survey made by the White House Conference on Child Health and Protection, involving a study of the home life of 1626 boys and 1552 girls of ages ranging between one and twelve years. Parents' answers to questions concerning the children's favorites in the home and the conditions under which jealousy was aroused manifested, with respect to the Freudian assumption of tendencies for the boy to be attached to the mother and the girl to the father, no outstanding sex differences in emotional attachment at any age level. About half of the boys and girls had no favorites, and the other half showed a somewhat greater tendency for both boys and girls to favor the mother. The tendency to have favorites decreased with age. The chief cause for jealousy in both sexes was the showing of affection by the mother for another child. Girls showed more jealousy than did boys and there was a reduction in jealous behavior with age. No evidence for the Freudian Oedipus and Electra complexes was found.

Thomas F. Vance (Iowa State College) reported results of observations on food selections by nursery school children, made through a period of one year including the summer term. The order of tasting and finishing the four foods in the main course was recorded. A high correlation of this order between different quarters was found. Meat and eggs ranked higher in general than vegetables, raw

were preferred to cooked vegetables, and the latter to puree. Food combinations were not eaten as readily as single elements served separately. The low ebb in appetite occurred in December and January, and in June and July. There was a positive relationship between the frequency of serving the several foods and their ranks as to preference. This, of course, it may be pointed out, cannot be taken as evidence of causal relations, since those preparing the foods have had experience with other children.

Joseph G. Yoshioka (Anthropoid Experimental Station of Yale University) reported results of daily measurements of temperature, pulse, and respiration rates of four young and two infant chimpanzees through a period of three months. The rank as to variability was found to be pulse rate, respiratory rate, temperature; but the ratio of respiration to pulse rate is relatively constant. The P.E. of the ratio, however, varied significantly, indicating a higher efficiency of this heart-lung coördination in some individuals than in others. The P.E. of the ratio was found to correlate positively with degree of variability in general behavior. The constancy of respiratory to pulse rate would doubtless be expected if one considers the sort of stimuli that arouse each of these activities.

Luton Ackerton (Institute of Juvenile Research, Chicago) presented results obtained by him and Minna Emch on the computation by five different methods (those proposed by S. L. Pressey, Merrill, G. Thomson, Woodworth, and also a subjective rank-order method) of the scatter on the same set of 100 Stanford-Binet test records. The ranks by subjective estimates were based on the ratings by four workers experienced in clinical procedures. Inter-correlations ranged from 0.66 to 0.97, and those between different pairs of raters' rankings, from 0.77 to 0.93 (means 0.87 and 0.85, respectively). The mean correlation of the equally weighted composite, subjective rankings with those by the other methods was 0.79, the lowest coefficient being with the Woodworth-Thomson measures. Since these latter were considered the best technique, it is recommended that in clinical practice the subjective method should be discarded and computations made by the Woodworth-Thomson technique.

E. J. Asher (University of Kentucky) reported results of applying the Myers Mental Measure to 363 children in 15 rural mountain schools of southeastern Kentucky, the National Intelligence Tests to 234 of these children and the Stanford-Binet and the Herring-Binet to 56 children. The median IQ were, respectively, 68, 72, and 73; and the IQ by the Myers test decreased with age from 84 at seven to 61 at fifteen. Since there was no significant difference between the verbal and the non-verbal tests and all gave obviously too low IQ, the conclusion was drawn that the tests were not suitable for children with such inferior environment. It should be added that similar results have been found on comparative racial tests; and also that IQ computed from different group tests will give very diverse results so that this method of computing IQ is not to be recommended.

D. A. Worcester (University of Nebraska) gave summaries of three studies of long-time practice effects on later intelligence test scores, one study on high school and two on college students. In the high school group significant increases in scores, due to previous practice, were found. The scores increased directly with the extent of practice, but with a negative acceleration. In the college group it was found on Alpha that a second administration brought significantly higher scores than the first; and, on the Ohio State University Psychological

Test, that experience with any other intelligence test at any time showed a significant effect. Such effects of practice are now well-known to experienced testers.

Noel B. Cuff (Eastern Kentucky State Teachers College) reported preliminary results of his experimentation with the best methods of scoring objective tests. When answers were written on a separate sheet, conveniently arranged for scoring, rather than on the test-sheet in the usual manner, he found it possible with the use of stencils to increase considerably the rate and the accuracy of scoring both true-false and multiple-choice test papers. Stencils containing mimeographed circles around correct answers were used.

The final session, held Thursday morning, was devoted to abnormal behavior and emotional maladjustments. Paul C. Young (Louisiana State University) reported an experiment on resistance to muscular fatigue in hypnosis. Thirteen college students served as hypnotic subjects and six as controls, the former being tested on the hand dynamometer first in the hypnotic and then in the waking state under conditions otherwise as uniform as possible, while the controls were tested both times in the waking state. Results seemed to show "no great increase in the ability to withstand fatigue in hypnosis; but all hypnotic subjects claimed to have felt no fatigue in hypnosis or immediately upon coming out of the hypnotic session during which they had been fatigued almost to exhaustion." Individual variations were apparently not changed under hypnosis. Some criticisms arose in the discussion as to details of the technique which seemed to be inferior to the technique found now in laboratory experiments.

Roy M. Dorcus (Johns Hopkins University) summarized briefly the psychological and neurological implications of "dissociation," and reported experiments comparing the galvanic activity of *Ss* in whom anaesthesia, anacusia, and amnesia had been induced hypnotically with the galvanic activity of *Ss* under normal circumstances. The lack of a difference between these two groups was interpreted to indicate that one of the meanings attributed to "dissociation" is a mere abstraction since there is no evidence of any breaking up of the neural pathways.

Trigent Burrow (New York City), discussing relations of "normal illusions" to "illusions of the insane," held that psychiatry gains little from attention to the patient's symptoms—false ideas, delusions and phobias, elations and depressions, etc.—these being "but reflections of an impairment that is deeper seated within the organism and which is equally characteristic of normal as of pathological individuals. This impairment consists in definite physiological tensions, alterations, and disturbances." He held that "experimentation under conditions of laboratory control" indicates that the psychopath suffers from a conflict between "those feelings and sensations which belong to the organism as a whole and those sensations which belong to the segment of the organism located within the cerebrum, with its secondary acquired ideas and images," a conflict that is social or phyletic in its scope. "Such a condition is perceptible and remediable only through recourse to physiological methods of approach and not through a program that attempts to exchange ideas for ideas and images for images." Unfortunately, the short discussion did not make clearer either the specific nature of the experiments or of the physiological methods referred to in the paper.

John M. Fletcher (Tulane University), in a paper on medical analogies in abnormal psychology, traced briefly some of the early borrowings of psychology from medicine, and pointed out that "in no system of psychology is there such a de-

tailed array of medical analogies as there is in Freudian psychology." This he held to be true of Adler's doctrine of compensation for inferiority derived from data secured in the practice of pediatrics as well as in Freud's diagnosis and treatment of hysteria. "Psychology therefore owes much to medicine," he concluded, but warned that "analogies do not afford proof," and are useful in cases "only by way of suggestion." The clearing up of some of these fallacies in these analogies will probably result in "some of our schools in abnormal psychology" going "the way of faculty psychology and associationism." It is probably charitable to class the Freudian psychoanalysis as 'psychology' even though some of its contributions in motivation (not new, however, with psychoanalysis) have been suggestive for psychological study. Psychology doubtless does owe much to medicine, but the 'therefore' in the context of the quotation above suggests (probably inadvertently) a too narrow source of the contributions of medicine and an unscientific one, in the main.

H. Meltzer (Psychiatric Child Guidance Clinic of St. Louis) reported results of an investigation of college students' adjustments in anger. The anger responses of 93 psychology students—58 women and 35 men—about one-half of whom lived in sororities or fraternities, were studied by a "controlled diary" method. The diaries yielded 393 anger responses for analysis. Data include conditions before the anger was aroused, conditions and sensed impulses in anger, duration of anger, and certain after effects, controls, and social organization of tendencies. Anger impulses were found to be closely related to organization of home, school, and community life, and to frustrations of one's activities; and the study of anger was found to be "a problem in social dynamics." The men reported 39% more anger experiences than the women. Sixty-four percent of the anger experiences reported by women were reactions to persons, nearly three times as many of which were defensive as aggressive. Only 36% of the men's anger reactions were to persons and over four times as many of these were defensive as aggressive. The general problem is suggestive of careful experiments with better technique. Certainly the last items mentioned in the results, for example, need careful study under conditions that are very well controlled.

The final paper of the last session was by J. B. Maller (Teachers College, Columbia University) on the character and background of the young men who were chosen candidates for the Edison scholarships during the years 1929 and 1930. These 39 high school graduates represent all the states and the District of Columbia. They "had been chosen on the basis of rigorous examinations" and were supposed to represent one of the highest ranking selections of high school graduates in the United States. It was found that the Edison group graduated from high school at 15.5 years of age, about 0.5 year younger than the average age of high school graduates through the country; their parents were chiefly of the professional and commercial group, only 5% being children of farmers or unskilled laborers. About 50% of them had scientists in their family, whereas the corresponding percentage for the other high school graduates is 34. The Edison group came from a higher-than-average social background and their interests are predominantly (by selection apparently) in science. They scored "very much higher in science and mathematics and somewhat higher on questions of personality" than the comparison group. They also, incidentally, took more time for sleep and meals (10.6 hours daily to 9.4) than the other group. Their favorite type of book was that which deals with science and invention, while the other group more

generally favored books on fiction. The data available to the reviewer are either inadequate or several of these differences may be accounted for on the basis of the fact that the Edison group of boys were selected rather rigidly according to a certain interest and had an environment which encouraged different lines of activity and daily habits.

At the joint dinner of Sections I and Q (Education) held in Kolb's Restaurant Tuesday evening, the vice-presidential addresses by E. G. Boring and by Leonard V. Koos on "The Physiology of Consciousness" (now published in *Science*, January 8, 1932) and "The Procedure of Investigation in the National Survey of Secondary Education," respectively, were given. The subject of the address by the president of the Southern Society, given Wednesday evening by H. M. Johnson, after the banquet at Hotel Bienville, was "Some Follies of Emancipated Psychology." He challenged the validity, according to the criteria of the sciences generally, of "a large body" of doctrine in modern psychology, contending that psychology when emancipated from philosophy "repudiated the rules of reasoning and standards of proof which are prescribed by Aristotelian logic and which govern modern science and common sense practice. For this logic it has substituted a special logic of its own in which equivocation is treated as a valid method instead of a fallacy." Illustrations with definite citations were given of spurious generalizations, of the confusion of assumed data with observational data, of illicit identification of "operationally defined concepts" with formally defined concepts, and of the confusion of resemblances with identities. It is certainly important that psychologists do not sacrifice accuracy and standard scientific caution, in order to get their "contributions" to press either for rewards of advancement in rank or for financial returns. There is serious need today of more careful and frank criticisms and evaluation of published results of research, as well as of positive depreciation of the mere quoting of recognized leaders in the science. It is not the authority quoted but the validity of the data themselves that bears weight. It is a question how this fact can be made weighty when scientific work is mixed with salesmanship and often motivated by personal financial gain.

At the business meeting of the Southern Society for Philosophy and Psychology, following Wednesday morning's program, Anna Forbes Liddell, Florida State College for Women, was elected president for the next year and Herbert C. Sanborn, Vanderbilt University, and J. F. Dashiell, University of North Carolina, were elected members of the Council. The new committees on the Teaching of Philosophy and the Teaching of Psychology did not make formal reports at this meeting and were continued with the following membership: H. C. Sanborn, (Chairman), A. G. A. Balz, Josiah Morse; and P. F. Finner (Chairman), R. M. Dorcus, T. K. Sisk, respectively. It was voted to accept the invitation of American University and to hold the next meeting in Washington, D. C., on April 14 and 15, 1933.

Peabody College JOSEPH PETERSON

THE FIRST INTERNATIONAL NEUROLOGICAL CONGRESS

The First International Neurological Congress took place at Berne, Switzerland, August 31st to September 4th, 1931. There were in attendance 881 members, representing 42 nations. The largest delegation, 161, came from the United States, but France, Germany, Switzerland, and Italy each had a representation

of about 100. The President of the Congress was Professor B. Sachs, of New York City. Papers were presented in French, Italian, German, and English, with no translation. There were numerous excursions, interesting exhibits, a number of moving picture presentations, and the usual conference out-of-session chats. The concern of this brief report, however, is to give something of a picture of the present activities of neurologists represented by the 234 papers included in the book of abstracts.

A general summary of the central themes in the papers of the conference may be found at the end of this paper. Although the categories are not mutually exclusive, and the papers often touched on several themes, each has been tabulated only once, and in the light of its major objective. The summary will serve to indicate, however, the general absorption of neurologists in physiological and pathological considerations which is to be expected of a branch of the medical profession.

It is interesting to note that our present understanding of disorders of the brain and nervous system is very largely in the stage of attempts to secure accurate descriptions and sound anatomical and histological knowledge. There were very few hopeful developments in therapy, but presumably the more careful techniques of diagnosis will eventually serve some purpose more constructive than simply the improved accuracy of prediction as to the rate at which the paralysis will advance, and the length of time before an autopsy can be performed.

The contributions of interest to psychologists vary, of course, with the psychologist. Pavlov gave a brief review of the development of 'neurosis' in dogs by strong excitation, collision of stimulus and inhibition, excessive tension of inhibition, and by castration. These were manifested in chronic lowering of excitability or of inhibition, and in chaotic activity. Szondi (Budapest) called attention to the fact that there are no essential symptoms of neurasthenia. Every subjective state mentioned may be matched in some other case by its opposite. He believed it possible, however, to distinguish two types, (1) the apathetic-asthenic, characterized by a flat blood-sugar curve and a hypo-function of the sympathetic system, (2) the excitable-pyknic characterized by a rise in the blood sugar curve and involving a hyper-function of the sympathetic and adrenal.

A number of the papers showed a rather naïve faith in localization of psychological functions. One, indeed, claimed to have established as long ago as 1909 that the center of consciousness was located near the third ventricle, and that all the neuroses, subconscious activities, hypnotic reactions, changes of mood, forms of dementia praecox, catatonia, sleep disturbances, epilepsy, obsessions, compulsions, and character traits must be explained in terms of modifications of this center. Of evidence of localization of motor functions there was no lack, but the modern point of view in psycho-neurology was approached by Krause (Amsterdam) in his report that while catalepsy is destroyed by loss of the motor area of the cortex it can be reëstablished after three to five months, the function being apparently taken over by other cells. He found that the size of the lesion was a very important factor in influencing the extent and permanence of the motor changes. The difficulties in interpreting evidence on the relation of structure to function were made apparent in many of the reports. Thus, two tumors may grow in precisely the same area, but if one develops rapidly and the other

slowly, the symptoms of the first may be acute and marked, those of the second may never appear. The effect of the cortical disturbance in bringing about anemia or engorgement of blood supply at other points brings an enormous number of variables into play. The effect upon pressure of the spinal fluid may be felt at points far removed from the original disturbance. Behavior changes are influenced, of course, by the personality make-up. Trauma is used as ground for a neurosis by some persons, while others react quite differently to much the same degree of apparent injury. The break-down in the area of compensation may be more serious than the original disturbance which set up the need for compensation. So the variables may be enumerated. It is small wonder that one reports a case with symptoms corresponding to tumor of the left frontal lobe while autopsy reveals it to the left parietal; another a case with a tumor apparently in the base of the brain but actually with a glioma of the temporal lobe; another a case of apparent frontal tumor but actually with a large hemorrhagic cyst in the left cerebral hemisphere and with internal hydrocephalus; a fourth, a case appearing to be a neoplasma of the left cerebellum while autopsy revealed glioma of the right frontal lobe.

Tests appeared rarely. Weisenburg of Philadelphia reports having found that normals make much the same type of errors as are made by pathological subjects on all the existing tests of aphasia, including Head's, and so has turned to the development of new tests in this area. One neurologist, who may here be nameless although the United States must own him, made the only mention of intelligence tests, and that in connection with an exceptionally naïve report that very dull children learn to talk at a later age than do normals. Minor (Moscow) reported a very simple and hopeful test of handedness as a result of his observations that the spread of veins on the back of the hand gave an anatomical basis for discerning genuine handedness, regardless of training. Oberholzer of Zürich seems to have found the Rohrschach tests of value in distinguishing the organic disturbance due to trauma from the secondary psychoneurotic developments, but in common with most of the reports, the data were not given in a form to permit statistical appraisal. The lack of statistical concern was perhaps most apparent in the paper which discussed sex and race differences on the basis of a total of 18 cases.

Psychotherapy was discussed in only one paper, and that by a Viennese who not unnaturally emphasized the serious limitations of such techniques. Glandular therapy was not often mentioned, and the tenor of the observations was not very hopeful. The obesity, somnolence, and mental retardation commonly connected with a hypopituitary condition was attributed in one very thorough review not to the gland but to pressure on the floor of the third ventricle. Another paper stated that no preparation of the secretion of the anterior-pituitary acidophilic cells supposedly responsible for structural growth, had been found suitable for human use. Another reëmphasized the fact that the function of the pineal is unknown and that the precocious puberty sometimes observed in connection with pineal disorders was not affected by administration of the gland and not reproducible on animals. Of considerable interest to psychologists was the observation (not the first of the sort, I believe) that tired muscle is restored to something like its original efficiency either by sympathetic stimulation or by contact with adrenalin. The action of hormones in the blood stream was clearly demonstrated in one paper which secured electrocardiagraph records from the

mother and the foetus, connected with the mother only by blood stream and not by direct nerve communication. When the vagus nerve on the mother animal was stimulated the change in mother's heart beat was almost immediate, and was followed ten or fifteen seconds later by a parallel change in the foetal heart beat.

Other phases of blood chemistry appear of considerable interest in disorders of personality. Timme finds that deprivation in the utilization of calcium (not necessarily in its availability) is associated with compulsive thoughtless, rapid, uncritical, and hence often unsocial behavior. This state is usually accompanied by high blood sugar, high blood pressure, and rapid fatigue. Another neurologist found a low calcium content of blood and spinal fluid in 150 cases of grave mental confusion. Dementia praecox and epileptics showed a normal pH, alkaline reserve, Cl and Ca in both blood and spinal fluid.

Two reports described methods of producing sleep. The first was electrical stimulation of the mid-brain of cats, which produced a drowiness so natural that the cats would not lie down on a damp bed to sleep but waited until a dry cloth was made available. In the other report, a profound sleep lasting three to six hours was produced by injection of a 5% solution of calcium chloride into the third ventricle of cats. Ergotamine had the same effect. Potassium chloride produced a state of agitation.

Students of problems of reading will be interested in some investigations pertaining to eye movement and word recognition. A lesion in the posterior half of the hemisphere, especially one that is supramarginal or in the angular gyrus, or which affects adjacent portions of the parietal, occipital or temporal lobes produced a type of eye-movement which was slow in its pursuit of moving objects, but relatively rapid in its return to and fixation upon the next oncoming object of the series. Lesions of the left supramarginal gyrus were reported by another speaker to be characterized by difficulty in repetition of words, difficulty in oral reading, verbal paraphasia, and the secondary disturbances in arithmetic and writing. A disturbance on the rim of the area striata was apparently responsible for a type of dyslexia in which the subject could grasp one symbol but could not coördinate one with the next and the next in a fashion to make reading possible. Disorders of motor speech, apart from verbal comprehension, were found in connection with tumors of the frontal spheroidal lobe, and with lesions in the Broca area. Of especial theoretical suggestiveness was the observation made during the course of the discussion by one German neurologist that a pathological subject, unable to name an object, does not respond at random. He is apt to give it a name that sounds like the correct one, or to name an object like the original in color, in size, shape, function, or otherwise associated.

The clinical psychologist finds after a week of discussion with the neurologists an increased alertness for organic disorders in his cases. He would always, of course, refer to a competent neurologist for examination any case showing exaggeration or diminution of any reflex, partial paralysis or anaesthesia, hypertonia or rigidity in some muscles, disturbed gait or posture, inadequate coördination, local headaches, restricted visual or other sensory fields, hallucinations involving any sensory channel. Even such symptoms as ready fatigue, aphasias, tendency to unwarranted euphoria or depression, marked alteration in personality, tendency to re-live scenes of earlier childhood, psychic automatism, restlessness, indifferent affectivity, drowsiness, increasing dullness or confusion,

vertigo, loss of sphincter control, and memory failures, while by no means usually caused by neurological disorders, were included among the evidences presented for the diagnosis of organic disturbances. The clinical psychologist would probably find, after the neurological diagnosis had been made by the help of the best modern techniques, that, if the patient were likely to survive, the job of training him to some sort of usefulness and enjoyment in life in spite of his inevitable handicaps, must needs be passed back to the psychologist.

In the total number of papers contributed Germany led with 29, Italy and America followed with 28 each, France with 22, Austria with 17, Czechoslovakia with 15, Russia and Sweden with 13 each, Switzerland, England, and Poland with 12 each; Holland with 11, Hungary with 7, Spain and Portugal together with 6, and all others with 9.

As regards the topics treated, the figures are as follows. Under the head of *pathology*, the cause, mechanism, or course of some specific nervous disease or disorder, there were 70 papers, Italy leading with 11, followed by Germany and Russia with 7 each, America, France, Poland, and Holland with 6 each; Czechoslovakia-Roumania-Greece[1] and Switzerland with 4 each, Sweden-Norway-Denmark[2] and Spain-Portugal with 3 each, Austria and England with 2 each, Hungary with 1, and all others with 2. The greatest number of papers under this head dealt with infections, toxins, and lesions, 27 in all, Holland leading with 5, Germany following with 4, Italy, America, and France with 3 each; Czechoslovakia and Poland with 2 each; Hungary, England, Austria and Sweden with 1, and all others with 1. Next in frequency came papers on trauma and post-traumatic neuroses, 16 in all; Italy, France, and Russia with three each; Germany, Switzerland, and Spain-Portugal with 2 each; England with 1. Fourteen papers dealt with atypical and unusual cases; Poland with 4, Italy with 3, Czechoslovakia with 2, and Austria, Russia, Sweden, Poland, Holland, and Spain-Portugal with 1 each. There were 10 papers on tumors, gliomas, and the like, America, Russia, and Switzerland presenting 2 each; Germany, Italy, and Sweden 1 each, and all others 1. Only 3 papers dealt with glandular disorders, one each from Italy, America, and Russia.

Diagnostic technique stood next to pathology in the number of papers dealing with it, 54. There were 23 papers on brain puncture, fluid examination, X-ray examination, ventriculography, encephalography, chronaxy, etc.; 11 from Germany, 3 from America, 2 each from Sweden and Hungary, and 1 each from Italy, France, Austria, Russia, and Holland. Sixteen papers dealt with clinical signs; gait, motor and sensory disturbances, pains, sweat, body-type, etc.: 3 of them from France, 2 each from America, Czechoslovakia, and England, and 1 each from Germany, Italy, Austria, Russia, Sweden, Poland, and Hungary. Five papers dealt with body chemistry: acidity, sugar, albumen, phosphates, Wasserman, Nonne-Appelt, N, Cl, Ca, etc.: 2 of these from Austria and 1 each from America, Czechoslovakia, and Hungary. There were 6 papers dealing with psychological means of diagnosis; tests of intelligence, special abilities and handicaps, perception, handedness, etc.: 2 of them from America and 1 each from Germany, France, Russia, and Switzerland. Four papers, one each from Italy,

[1]This group hereafter indicated in the text by *Czechoslovakia*.
[2]Hereafter in the text indicated by *Sweden*.

France, Czechoslovakia, and Spain-Portugal, came under the head of miscellaneous combinations of technique.

Under *general anatomical, physiological, and histological topics* 22 papers might be classed, 3 from Austria, 2 each from Germany, Italy, America, Sweden, Switzerland, England, and Poland; 1 each from Roumania, Russia, and Spain-Portugal; and 2 from other countries.

Tone and posture were discussed in 19 papers: 4 American, 3 each from France, Roumania, and England; 2 each from Italy and Holland, and 1 from countries classed as miscellaneous.

Eighteen papers dealt with *categories, classifications, comparisons,* and *outlines:* 4 of them from France, 3 from America, 2 each from Italy, Austria, and Spain-Portugal; 1 each from Germany, Russia, Sweden, England, and Poland.

Seventeen papers dealt with *therapy.* Eight of these had to do with surgery; 2 each from America and France and 1 each from Germany, Italy, England. Four were concerned with radiation, 1 each from Germany, France, Sweden, and England. Two had to do with drugs, from Italy and America respectively. One paper from Sweden dealt with malaria, and 1 from Austria with psychotherapy. Preventive sanitation and hygiene were the subject of 1 paper from Sweden.

Thirteen papers had to do with *localization of function in brain areas:* 2 each were from Germany, Italy, Czechoslovakia, Switzerland, and miscellaneous countries respectively and 1 each from America, Holland, and Spain-Portugal.

Finally, there were 21 papers classed under *miscellaneous,* including medical training, laboratory experiments, embryology, anthropometry, etc.: 5 from Austria, 4 from Italy, 2 each from Germany and Switzerland, 1 each from America, Czechoslovakia, Russia, Sweden, Poland, and Holland, and 2 from countries classed as miscellaneous.

Teachers College
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BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

A B C de la Graphologie, I & II (*Documents*). By J. CRÉPIEUX-JAMIN. Paris, Félix Alcan, 1929. I, pp. 357; II, pp. 368.

All readers who desire information concerning graphology at its best should read this lucidly organized book by the veteran graphologist, Crépieux-Jamin, and should study carefully the volume of documents that accompanies it. Even the most skeptical critic of graphology must acknowledge the curious problems projected by such an exhibition of graphic individuality. As an illustration of the inexhaustible variety graphic forms may assume, the author cites the possible variations that may occur in the writing of even one letter. It is possible, he asserts, to accumulate a thousand, ten thousand, or a hundred thousand specimens of the same capital letter without encountering any that are identical with each other.

The first and fundamental need for the student of handwriting in the face of such diversity is descriptive classification. Such classification involves a systematic procedure which slightly resembles systematic work in botany or zoology. It is, of course, keyed much less objectively. The author proceeds from seven genera, which give merely an outline of his program, to the species which are much more basal. Generic characters include speed, pressure, form, direction, dimension, continuity, and order (*l'ordonnance*). These genera are divided into 175 different species, each of which is described and illustrated. The species are differentiated in terms of a general form-quality of movement which one must *learn* to see. Instructions are given by means of which one may re-live a movement. Subordinate to the species are the modes, or *petite* signs of graphology, characteristics which the amateur sees first, and which are misleading. The more narrowly a sign is specialized, says Crépieux-Jamin, the less it is adapted to represent a fundamental trait of character. There are no particular independent signs of the comprehensive graphic pattern. One must begin with the species and not with the details.

Some of the species, described as qualitative, are, moreover, of much more fundamental significance than others, for example *l'écriture inégale* with all its diversity of nuances. Important, too, is the general quality which is discussed under the two headings *l'écriture organisée*, and *inorganisée*. In the latter discussion one approaches the problem of the development of writing from the immature to the mature hand. One may study the organization of an individual handwriting in the experience of the child, or see the reversed process as it occurs in the failure of control as old age sets in and progressively disorganizes the motor expression. There are other dominant categories which cannot be detailed here, except, perhaps, to state that in common with other students of graphic movement Crépieux-Jamin stresses speed and dynamic qualities. It is the habitual rhythm and degree of activity that the graphologist is seeking to penetrate. It should, of

course, go without saying that all graphologists of standing insist upon ample documentation in an investigation, and are on the alert to recognize the accidental modifications of writing which occur under such conditions as writing in a cold room, on a moving train, or with different kinds of writing implements, on poor paper, or in a restricted space, and the like. The individuality that persists in spite of surface changes is what is sought by the graphologist.

From the descriptive side, Crépieux-Jamin's presentation is very valuable. But when one turns from description of the various patterns of highly individualized writing to a psychological interpretation of them in terms of personality one finds oneself on slippery ground. The new graphology no longer relies upon the graphic signs of the older graphology. It is interested in the form-quality of the writing taken as a whole, and not in the identification of unrelated details, the synthesis of which is a bewildering matter. A little has been done to test by objective methods the validity of the graphic signs of the older assumptions, but scarcely any attempt has been made to experiment on form-quality of writing although Binet made some approach to the problem. Crépieux-Jamin, while rejecting a purely analytical treatment of handwriting and emphasizing the need for a synthetic grasp of the whole, has little faith in so-called intuition and no faith at all in the amateur graphologist. He calls for prolonged training in the perception of form, and frequently relies upon an esthetic evaluation.

In the absence of any evidence proving or disproving the fundamental assumptions of the new graphologist—namely, that the form-quality which distinguishes the graphic movement will also be found distinguishing the personality taken as a whole—it is possible only to call attention to certain points upon which experimental evidence is actually at hand. Crépieux-Jamin treats reversed writing, or the back-slanted hand, in a fashion much more conservative than that of the older graphologists. He realizes that it may proceed from a number of different causes and cites some interesting statistics as regards the frequency with which it occurs among school children in France. He holds it to be more common among adolescent children than among adults. When it appears in the writing of adults he is inclined to interpret it in accordance with the old-time view as an evidence of emotional repression. Experimental work has, however, shown that a back-slanted hand is, in many cases, evidence of crossed dextrality, that is, opposition between the dominant hand and eye, or of latent sinistrality. It is not impossible that one may find temperamental patterns built upon inhibitions and difficulties due to conflict in kinesthetic impulses. Such, however, remains to be proved. In the meanwhile the explanation of back-slanted writing in terms of latent sinistrality is much more specific than the graphological explanation. I cite this as an illustration of the difference between the graphological and psychological approach to the same problem. In the same way, in the author's discussion of the graphological significance of misspelled words, he fails to reckon with statistical data available in the field of spelling-research.

One would like to see graphology and psychology come into more intimate relationship. The graphologist is, for the most part, ignorant of recent attempts to handle the problems of personality scientifically; the psychologist is blind to the nuances of graphic form. Can ground be found for a common experimental program?

University of Wyoming JUNE E. DOWNEY

Human Nature: A First Book in Psychology. By MAX SCHOEN. New York, Harper & Bros., 1930. Pp. xviii, 504, 3 pl.

Frankly disclaiming all pretensions as a treatise, this book sets out with the avowed aim of establishing an *entente cordiale* with the reader. This objective it would appear fairly to have attained. Fluently written and well printed, with attractive type and ample margins, it prefaces each section with provocative summaries and suggestive questions. The elementary facts of the nervous system are concisely furnished, with accessory cuts of unusual clarity and bold lettering. Habit, instinct, emotion, personality—ever popular themes—receive fairly representative treatment, with copious citations from social, abnormal, and comparative psychology. Accounts of experiments in the psychology of music and questionaries on various traits are inserted—in such detail as to vitiate, one would suppose, the future scientific value of the latter.

These merits are achieved, however, at the expense of serious omissions and foreshortenings of content, jeopardizing the value of the volume as a college text. Though avowedly representing both 'structuralism' and behaviorism, and citing exponents of 'mind' (Russell, C. K. Ogden, Köhler) as frequently as Pavlov or Watson, the author does scant justice to the typical experimental attack of the older, stabler tradition—the use of hypothesis, standardization and ordered variation of conditions. In the fields of feeling, action, and imagination, facile verbal solutions or opinions replace exact analysis of present-day enigmas; and rationalization and defence mechanisms are accorded fuller space than 'thinking' proper. The doctrines of heredity and 'unit traits' are cavalierly treated in an early section, only to be quietly readmitted as basic truths, when needed later to explicate the variants in musical talent and in personality.

The logic of the author's terminology is often difficult to decipher. Wide departures from current usage in the handling of such stock terms as *attribute, inference, accommodation, psycho-physical measurement* occur (pp. 184, 16, 187, 31), with no discoverable gain. Cut to the capacity and tastes of the lay reader rather than the student, the treatment on the whole is of a nature to repel rather than attract the type of recruit sorely needed in the psychological field today—the individual of curious yet critical and constructive bent.

A booklet of clever and suggestive "Learning Exercises," cast in the mould of the familiar 'true and false,' multiple choice, and similar forms, has been devised by the author and his staff to accompany the volume. There are suitable exercises, designed for individual use by the student, for each of the main topics, running in all to 38 pages.

Cornell University ELSIE MURRAY

Geniale Menschen. By ERNEST KRETSCHMER. Berlin, Julius Springer, 1929. Pp. viii, 254.

In *Geniale Menschen* Kretschmer has given us a scholarly and systematic treatise on genius, written from a scientific point of view. By his repeated emphasis upon objective data and his substitution of scientific method for purely philosophical speculation, the author has made a significant advance in the direction of a scientific psychology of genius.

The problem of the study is "to demonstrate the laws governing genius and its inheritance, and thereby present a plastic true-to-life likeness of creative personali-

ties." The treatment follows the lines suggested in the chapter on "Die Genialen" in *Körperbau und Charakter*, 1921. The scope, however, is much broader, including all the major problems of genius, of which the question of psychosomatic types is only one.

Kretschmer's criterion of genius is two-fold: (1) the possession of certain unique qualities of personality, and (2) a positive reaction of the social group to these qualities. The essence of genius is a specially structured mind, which produces positive vital values, that bear the stamp of an unique and unusual individuality.

The main body of the book is divided into three parts. The first, entitled "Laws" is systematic, dealing with particular problems of genius. The first chapter is devoted to a discussion of the relations between genius and insanity. Kretschmer expressly denies Lombroso's contention that all genius is insane, yet in the end his own position comes very close to Lombroso's. The formula for genius is high intellectual endowment plus the *demonic* element. The *demonic* is defined as that element which finds expression in the strange mental activities, the unusual ideas, and the uncommon sorrows, characteristic of genius. Precisely how *demonic* traits differ from psychopathic traits, Kretschmer never makes clear. On the contrary, he tells us that the demonic and psychopathic elements are very closely related, sometimes inseparable. Some *demonic* traits are identical with psychopathic traits. Nevertheless, there is no necessary 'inner' relation between insanity and genius. The typical genius is insane, but his insanity is not essential to his genius. The argument is not convincing. Whatever might be gained by postulating a *demonic* element seems to be lost in the author's failure to draw any clear distinction between it and the psychopathic element. In the absence of any criterion of the *demonic*, Kretschmer's position approaches Lombroso's in fact if not in words.

Other problems discussed in Part I are abnormalities of the sexual impulse, constitutional types, the inheritance of talent, and the relation between genius and race. One is led to believe that the majority of geniuses have some form of sexual abnormality. In theoretical interpretation of sex functions, Kretschmer leans heavily upon Freud. The author's theory of constitutional types is briefly restated, and the reader is referred to *Körperbau und Charakter* for a more detailed discussion of the subject. Regarding the inheritance of talent, Kretschmer suggests that the high frequency of genius among the offspring of unions of diverse family stocks is due to the fact that such unions produce individuals of complicated psychological structure, inner contrasts and conflicts, restlessness and instability. As for the relation between genius and race, the author proposes a theory based upon the geographical distribution of genius. Genius and the highly developed culture which invariably goes with it occurs most frequently in zones of racial mixture, in which there is a high degree of intermarriage between two diverse races. Within the domains of the Nordic race, the highest cultural centers appear, not where the race is purest, but where it is highly mixed with other gifted races, especially the Alpine. Thus Kretschmer's "objective statistical method" leads to a refutation of the popular theory of Nordic supremacy. Unfortunately, the statistical evidence is not included, so that it is impossible to judge the adequacy of his method of selecting data. Racial types are not identical with constitutional types. There are various constitutional types in every race, with some one type probably predominant. The Nordic race, for example, shows a large proportion of the asthenic type of physique; the Alpine race, a large proportion of the pyknic.

In analyzing the physical and mental traits of the peoples in the zones of Nordic-Alpine mixture, Kretschmer attempts to demonstrate a correlation between ratio of Nordic-Alpine blood, type of temperament, and form of genius. In regions where the Nordic component is predominant, the temperament of the people is relatively 'cooler,' and here we find the center of political talent and techno-scientific development, (England, northern Germany, and North America). In regions of Alpine predominance, on the other hand, we find a 'warmer' temperament—the center of artistic culture, (middle-southern Germany, middle-upper Italy, and France). These generalizations are interesting, but one misses corroborative statistics.

In Part II, certain important trends and types of genius are illustrated by reference to the lives of famous geniuses. Goethe's life is cited as an example of periodic fluctuations in the productivity of genius. The scientific genius is typified by Robert Mayer; the heroic leader of men, by Bismarck, Luther, and Robespierre; the mystically inspired genius, by Hölderlin; the prophet, by Rousseau. The author pauses briefly in his portrait painting to argue that the phenomenon of genius is limited almost exclusively to the masculine sex. A few women, he grants, have done original and creative work, but they were essentially mannish. "The great women were great because they were great men," is his conclusion. Queen Elizabeth of England, Christine of Sweden, and the German poet Annette von Droste-Hülshoff, are presented as evidence.

The portrait collection of Part III has a brief explanatory preface. Philosophers show a strong tendency toward the leptosome (asthenic) type of physique. Athletics and pyknics seldom occur. Among natural scientists, on the other hand, we find pyknics and mixed forms in abundance, especially in the biological sciences. In the more abstract sciences, such as mathematical physics, the leptosomes predominate. Here again one feels the need of statistical evidence in support of the generalizations. The portrait collection itself is excellent, adding considerably to the value of the book. It contains pictures of all those geniuses who have received particular attention in the text and many others. Strangely enough, there are four women among them. But perhaps they all had mannish temperaments! The pictures are grouped according to type of productivity under five headings: (1) philosophers, (2) physicians and biologists, (3) dramatists and pathetics, (4) romanticists and stylists, (5) realists and humorists. There are 80 portraits in all.

Smith College E. Frances Wells

A Primer of Aesthetics: Logical Approaches to a Philosophy of Art. By Louis Grudin. New York, Covici, Friede, 1930. Pp. xvi, 247.

Beauty: An Interpretation of Art and the Imaginative Life. By Helen H. Parkhurst. New York, Harcourt, Brace & Co., 1930. Pp. xi, 336, 47 pl.

Of these two volumes, the latter skirts more nearly the psychologist's domain. Essentially an elaboration of a theosophical formula—the duality of the cosmos—it translates all beauty into a juxtaposition of antitheses (movement *vs.* permanence, order *vs.* chaos, unrest *vs.* stability). Art is, as it were, a microcosm reflecting or harmonizing the conflict inherent in nature and human consciousness. There is little explicit reference to either psychological or philosophical theory, and the efforts of the experimentalist are consistently ignored. Considerable space, however, is devoted to enthusiastic and suggestive cataloguing of the sensuous qualities with which the artist works; to eloquent inventory also of the permutations of

time and space and movement on the adroit handling of which the charm of sculpture, music and architecture depends. A wide familiarity with the various fields of art and of art criticism is apparent, and the text is enriched with reproductions of classic works of art, and photographs of modern buildings and of natural objects. The treatment of form and of artistic creation is, however, disappointing, the author lapsing too often into a mystical or dithyrambic vein. Few writers in this field, it may be added, command a more flexible and extended glossary of all the nuances of mood and feeling.

Grudin's text, in marked contrast, maintains a deliberate aloofness from the concrete and specific. Inspired, apparently, by the poet-critic Valéry's *Varieté*, the author sets out to forge an idiom of logical abstractions, unique to aesthetics, which shall redeem this field once and for all from the intrusion of the literary critic or the metaphysician. The problem of aesthetics is a problem of symbolism, of meanings in a set contextual field. Psychological aid is explicitly repudiated as irrelevant (this position grows more intelligible as one proceeds and the naïveté of Grudin's psychological concepts becomes apparent). Part I grapples with Russell and Bergson, with verbal logic and space and time and meaning. In Part II (Introduction to a Theory of Aesthetics), at the close, current views receive brief and sometimes trenchant criticism. Definitions of art as illusory, intuitional, utilitarian and moralistic, an escape phenomenon, an aberration, a function of the ego or a specific order of behavior (the latter derived from the limited context of 'the laboratory world of psychology') are all dismissed as tending to "reduce beauty to one or another set of attributes, without suspecting that beauty escapes reduction to any set of attributes whatever . . . but is definable only by means of a conjugation of such reductions through a series of their transformations." One is left with the impression that the initial optimistic promise of the text, 'to establish aesthetics for the first time as a field of authentic and verifiable knowledge,' remains unfulfilled.

Cornell University ELSIE MURRAY

Ensayo de Piscologia de la Adolescencia. By E. A. CHAVEZ. Mexico City, Secretario de Educacion Publica, 1928. Pp. 482.

This volume is written by a man who was intimately connected with the educational affairs of Mexico for about thirty-five years—sub-secretary of Education, rector of the National University, director of the Facultad de Altos Estudios, etc. It is he to whom James Mark Baldwin dedicates his *History of Psychology* as to "the great patriot and scholar." Chavez is also a translator of Titchener's *Text-Book* into Spanish. The present essay is something like a collection of independent sermons which draw on a variety of sources. Like a preaching philosopher, the author does not care about concrete material and experiments, tables, surveys, or case studies. His book is a message of good will "directed to the peoples of the earth, to classes of the nations, to parents and children." The world is in a state of moral anarchy, "and it is the duty of the present day educator to show the dangers and to put the present generation on the single right track."

The titles of the four large parts are as follows: (1) Adolescence and Life, (2) Inference and Corollaries, (3) Cardinal Phenomena of Mental Life, (4) The Highest Structures of Psychic Life. The factual material of the first two parts approaches the typical layout of a textbook such as that by Pechstein and Mc-

Gregor, and the data are almost entirely from non-Mexican sources. Only a few items refer to Uribe y Torosko and to Santamarina, for example, when the prepuberal increase of weight in girls is represented as being about one and one-half year earlier in Mexico than in cooler countries. There is also a section on the recent history of secondary education in Mexico City. The author seems to favor very definitely the separation of sexes in school life without any artificial separation outside of school. The third part of the book reveals the definite leaning of the author towards McDougall's hormic postulates. The formation and dynamics of Freudian complexes is explained as the tendency of unfinished acts toward "closure" in the sense of the Gestaltists. Superior forms of love, the question of sex education, and a criticism of the "so-called philosophical teaching" which unjustly, according to the author, discredits the Catholic Church, are interesting features of the discussion. The drive toward atheism is said to be motivated by the aversion toward restrictions which good morals impose upon people; and the overthrow of religion generally, to be followed by the development of low grade superstitions. Such superstitions are explained psychologically by the author on the principle "that when a more noble and higher function is affected it is replaced by the lower functions which reach a greater development."

It is characteristic both of the esteem which the author is enjoying in his country and of the great tolerance of the Mexican Government today, that Dr. Chavez's essays were published by the Department of Education. There is not a word, however, on the "school of action" which was proclaimed as the official 'school' in Mexico; neither was mention made of the psychological laboratories founded during the administration of President Obregon.

Lewis Institute, Chicago D. Boder

Die Psychologie der produktiven Persönlichkeit. By Paul Plaut. Stuttgart, Ferdinand Enke, 1929. Pp. viii, 324.

A methodological study of the principles and methods of the psychology of art involved the author in problems of psychography, and the accumulation of materials which suggested the need for a psychology of productive personality. The author formulates this in detail, although he regards his work as tentative and perhaps as a mere framework for future investigations. Creative personality—the man of genius—has always intrigued the professional psychologist, as well as the layman. Plaut's work combines the best features of previous discussions in this field. He does not rest satisfied with mere concepts of genius, talent or intelligence, but investigates concrete personalities displaying creative abilities. He appeals to the living genius for data. Since art and science are particularly favorable to the appearance of productive activity, the author seeks his material among scientists and artists who answered questionaries concerning their motives and methods of work. Plaut's book, however, is very comprehensive and he prefaces the results of his psychographic studies with a historical survey of genius and intelligence. He shows how the mythological notion of a genius or external spirit connected with man in his exalted moments is partially transformed by Plato into a conception of psychological endowment, in which most subsequent ideas of genius were present in germ. Later Greeks reduced it to a function of reason and no progress was made until the Renaissance. From that time, French, German, and English aestheticians and several of the French encyclopedists elaborated con-

ceptions of genius varying in detail but reducible to the two categories of rationalistic and mystical. The author dismisses the literature on the relation of genius to madness as irrelevant to the psychology of productive personality. He criticises the whole literature on the psychology of genius for its failure to develop anything more than an abstract and dehumanized concept indistinguishable for practical purposes from the mythological notion which was its source.

Intelligence is accorded a much briefer treatment, and the definitions and discussions of German investigators, such as Ziehen, Ebbinghaus, Meumann, Stern, and Lipmann receive most consideration. The influence of temperament and character on intelligence is duly recognized and the author offers a comparative study of intelligence types. He concludes that the psychology of intelligence has erred in the opposite direction to that of the psychology of genius. It suffers from excessive analysis resulting in the dissolution of personality into its elements. This is dehumanization of another kind. Neither the students of genius nor those of intelligence present a psychology of personality. This can only be worked out in terms of what seems to be a configurational psychology.

This brings us to the author's own contribution to the study of productive personality. Modern methods of personality study such as the individual psychology of Adler, the mass psychology of Moede, the type theory of Kretschmer, and the personalistic and allied methods, are stated and criticised, after which the psychology of the productive process in scientists and artists is elaborated in the light of such criticism. The psychology of the scientific personality, as expressed in natural science, philosophy of law, history, and religion, reveals criteria of productivity which are strictly normal, in spite of the fancies of the psychoanalysts. Brief chapters correlate the views of eminent scientists on productive types and processes. Artistic creation is the subject of more extensive treatment. Leading poets, painters, sculptors and architects contribute detailed material on the experience of the artist, and the sources of his mastery of aesthetic form. Sections follow on the typology of artistic creation and the nature of the processes involved. The author finally concludes that productivity emerges from a harmony of personality and its objects. It is a function of the total *Gestalt*. In activity, or will, lies the final formula of productive personality.

The final section of the work presents the views of contemporary artists and scientists on their personal methods of creation. Most of the writers are Germans, but there are contributions by Knut Hamsun and Miguel de Unamuno. Many of the contributions are of great interest and several contributors are frankly skeptical of the value of experimental psychology in this connection. In summarizing a vast mass of data, raising fundamental problems of criticism, and offering the beginnings of new approaches, Plaut has produced a work which will be indispensable to all students of personality.

University of Cincinnati CHARLES M. DISERENS

Wirkungsformen der Übung: Beiträge zur Psychologie der Arbeit. By BENNO KERN. Munster, Helios, 1930. Pp. ix, 508.

This monograph deals with the influence of practice on performance, and is based on the author's experience with aptitude testing during the war, and later with the students in the Westphalian schools. The first part is a theoretical discussion of certain obstacles encountered in performance testing—differences in

subjects' mental set, instability of initial performance, fatigue, differences in practice improvement of individuals relative to one another, etc.—and methods of controlling them. A discussion of the pertinent psychological literature, mostly German, accompanies each topic. There is very little that is new and much is omitted.

The second part describes a series of experimental studies on practice effect by the author and his collaborators. The justification for them is hard to discover. The groups of subjects are small and the tests limited in scope. Correlations are worked on less than ten cases and sweeping conclusions drawn therefrom. It is concluded on the basis of ten cases that girls are more accurate than boys. In arranging the successive studies in this series, the author was evidently trying to vary the number of trials at a sitting, frequency of sittings, length of rests between, and age and sex of subjects, in some unsystematic way. The results are presented graphically, demonstrating individual differences in the rapidity of the attainment of a stability of performance level.

The following conclusions are drawn: (1) the ontogenetic law of practice—initial efficiency rises with age, but practice effect decreases with age; (2) the differential law of practice—within any age group low initial efficiency is associated with large practice gain, and vice versa, due to the operation of initial inhibitions; (3) the law of practice consequence—the function is affected both as a whole and in parts; and (4) the limits of improvement—these limits are seldom reached, and an individual's initial level is no criterion of his stable ability. Hence performance tests can at present do little more than roughly to separate good and poor groups, without accurately diagnosing individuals. Certain improvements in procedure are suggested, such as arousing the will to improve, by group rivalry; introducing practice, spread over a long stretch of time, by utilizing alternate forms of tests until a more stable level is reached.

In fairness to the author, we must admit his central criticism, that unique samples of performance are never very reliable measures of ability, and that practice changes relative levels; although as Thorndike and others have shown, it is quite as likely to exaggerate preëxisting differences as to obliterate them. But did any psychologist ever naïvely assume that such samples of performance had any more than a rough classificatory significance? The point is that a test is only useful as a time-saver, and has no advantage over an observation of efficiency on the job itself, if the testing must be carried over several weeks. As to the author's ten years' research program, it was more or less gratuitous, since he has discovered nothing which is not already familiar to students of the practice function, as a result of more systematic experimentation. The five hundred pages could well have been reduced to fifty.

University of Chicago ARTHUR G. BILLS

Differential Manipulation in Children. By JULIA HEIL HEINLEIN. Comparative Psychology Monographs, 7, 1930, (no. 33), 1-121.

This is an investigation of the preferential 'handedness' in various motor activities of pre-school children, in relation to certain training effects. Observations were made under controlled situations as well as in daily activities of the children's own choice. Mixed types of individuals are now well-known, whatever may prove to be the respective rôles of germinal and of training factors in their

acts. Downey found by classifying adults and children according to the Rife scheme that these two groups show similar distributions of dextrality types.

The monograph in hand presents results on 24 children of ages 2 to 6 years (16 right-handed, 6 left-handed, 2 ambidextrous) in three different experiments: (1) offering equal opportunity for the use of either hand (marble-board test), (2) imposing inconvenience on the use of the preferred hand (in a form-board test), and (3) requiring rapid, spontaneous choice of the hand to be used (children were to run and pick up some object and bring it as quickly as possible). Different variations in the tests and detailed analyses of the responses are considered, as well as effects of practice on changes in handedness. The adequacy of the tests were judged by the degree of agreement of their results with the reports of handedness by parents. In (1) and (3) above, a pronounced tendency by both right- and left-handed children to use the preferred hand was found; but less consistent results came from (2). In all three experiments considerable variability in degree of bias was noted, convenience being an important factor.

Twenty-one children were also trained on a wooden peg-board to set pegs of different colors (B, O, G) one at a time, as quickly as possible, into holes in parts of the board having the same color. Each child had one practice with the non-preferred hand each day for 5 days, and also a beginning and an end test with the preferred hand. Improvement in time scores was shown on the non-preferred, practiced hand by both the right-handed (17 *Ss*) and, less certainly, the left-handed (4 *Ss*) group. The left-handed group, however, showed greater speed with the right hand on the first day than with the left hand. Moreover, the non-practiced (right) hand of the former group showed an improvement approximately equal to that of the practiced (non-preferred) hand. This may be due in part to transfer, to practice in the tests, to general maturity, or to certain irregular factors. One might expect the non-preferred hand to improve more than the preferred hand, even with equal practice. The left-handed group was too small for any serious consideration of results. This applies also to other experiments on training against dextrality tendencies, which therefore need not be considered here. Motivation in these experiments seems not to have been well controlled, due to several difficulties considered by the author. The results of observations on dextrality in uncontrolled activities agree closely with reports by parents.

The monograph could have been very much clarified and reduced in size by better tabulation and statistical handling of results, and by elimination of many irrelevancies and much confusing repetition. Its contributions, as it is, are not obvious. J. P.

Common Annoyances: A Psychological Study of Everyday Aversions and Irritations. By HULSEY CASON. Psychological Monographs, 40, 1930, (no. 182), 1-218.

After arranging two lists of 239 and 268 annoyances, collected from 659 subjects, Cason presented them for scoring to 625 and 378 subjects, respectively. From these data he obtained an annoyance score for each stimulus-word, and also an annoyance score for each individual tested.

His method of scoring both the stimulus-word and the individual records is identical. Since in both cases his range of possible scores falls between zero and thirty, some of his comparisons are more significant than they appear at first glance. For methods of comparison, Cason divided his subjects, running in

years of age from ten to ninety, into four age-groups for each sex. In no case did a group contain more than one hundred and five subjects, and eleven of his sixteen groups contained fewer than seventy-five cases. Such small groups lessen the validity of his conclusions, particularly in an introspective test of this type.

More than half of the monograph is devoted to a discussion of these annoyances, based to some extent on data collected from some five hundred subjects. These data are not presented for examination, nor can we assume that they came from those subjects who scored in the test.

From his original data, Cason concludes that human behavior constitutes over one half of our annoyances, non-human things and activities, combined with clothes and manner of dress, account for more than a third, while only about one eighth of our annoyances are due to either alterable or persisting physical characteristics of people.

No attempt was made to check the reliability of the test by having it scored a second time. It is unfortunate that the raw scores are not translated into standard deviation units or into percentile tables, and that no attempt is made to prepare diagnostic tests from the material collected.

Alabama State College MARIE HACKL MEANS

Heredity in Man. By R. RUGGLES GATES. New York, The Macmillan Co., 1929. Pp. xiii, 385.

The book is a mine of information concerning researches in heredity in man. In fact, at times it resembles an abstract journal, in that the various researches are not always searchingly criticized nor brought together into any synthesis permitting a final or unified view. In justification the author could no doubt truthfully plead that in many, if not most, cases our present knowledge is too meager to justify any evaluations even approximating finality.

Although the author expresses himself in the first chapter as believing in statistical or biometrical studies as well as in the analysis of pedigrees, only a smattering of studies of the former sort are included, and these few are not always analyzed as critically as might be. Throughout, the tendency is to assume Mendelian inheritance, but the author carefully avoids conclusions as to the exact mechanism of inheritance when not warranted by his facts.

The psychologist will learn little that is new concerning the inheritance of mental differences—only one of the sixteen chapters dealing with this subject. The remaining chapters deal with the inheritance of physical traits such as eye color, blood, abnormalities and diseases, and with racial crossing. The psychologist will, however, find a number of very interesting suggestions and discussions, as, for example, the discussion of the relationship between available knowledge of the general and specific factors in mental ability, on the one hand, and that of the mechanics of inheritance of mental traits on the other. Particularly in the discussion of the inheritance of 'musical ability' does he bring out the handicap under which the geneticists labor in attempting to determine the nature of inheritance of a trait which is not a unitary trait but a composite of an unknown number of factors. "The study of the inheritance of musical ability thus appears to be swamped at present in the welter of components into which it has been analyzed."

University of Illinois HERBERT WOODROW

Über das Wesen der Beanlagung und ihre methodische Erforschung. By THEODOR ZIEHEN. Friedrich Mann's Pädagogisches Magazine, Heft 683. Langensalza, Hermann Beyer & Sons, 1929. Pp. 88.

In this pamphlet Ziehen sets forth a psychological analysis of endowment (*Beanlagung* in the widest sense or *Veranlagung*) distinguished from talent (*Beanlagung* in the narrow sense or *Begabung*). Such an analysis he holds is the proper basis for the construction of tests of potentiality. He then states 15 principles covering the procedures to be followed in the systematic examination of school children and the development of better tests for the determination of vocational and educational aptitude. Finally, he gives examples of such tests in ten fields from Mathematics to Volitional Energy. Under each head several analytical test procedures are described with comments which suggest extensive experience and keen insight on the part of the author. This last part is especially rich with practical suggestions for research in this field, where as Ziehen rightly says "out of more than 150 recommended tests scarcely two dozen are satisfactory."

Points of outstanding psychological interest are the author's insistence on analysis, well summarized in his statement, "a general intelligence (general ability, Spearman) does not exist." In part this may be explained by his rejection of correlation coefficients as adequate means of representing mental relationships. He holds that psychic performances cannot be treated numerically as additive and divisible quantities. In this and in his frequent use of combination as a basic factor in all perceptual and conceptual activities, he seems to lean toward the Gestalt point of view quite in contrast to the emphasis on analysis just mentioned. He favors individual examinations and trusts the psychological training of the examiner more than test standardization, although he urges the importance of age norms.

The monograph is evidently a summary of psychological methods of determining potentiality based on wide clinical experience and close acquaintance with an extensive international literature. Evidence for the statements advanced and statistical data on the tests described are provided only in the bibliography of 67 references, chiefly to German periodicals.

University of Oregon HOWARD R. TAYLOR

Grundfragen der Wahrnehmungslehre. By PAUL FERDINAND LINKE. Second edition. Munich, Ernst Reinhardt, 1929. Pp. xxvi, 430.

This work may be appraised best in comparison with textbooks. In general the latter aim to present the fundamentals of psychology which should be the common basic currency of the science for every student. But this aim is almost without exception rendered futile by the systematic convictions of the writer. Thus textbooks tend to become special expositions of, and arguments for, a particular system like introspective atomism, behavioristic atomism, functionalism, *Gestalt*, etc. The selection of experimental material in each such book becomes prejudiced and inadequate, and logical consistency together with considerations of historical development distort the presentation. This may be a venial offence, inescapable perhaps because of the immature character of psychology. But it is possible that the offence may be more traditional than inescapable; for in textbooks on physics and chemistry and even biology fortuitous impressions and con-

victions are not so frequently permitted to interfere with the presentation of fundamentals, and a fair uniformity is achieved, at least in the elementary works.

The especial virtue of Linke's work lies in the circumstance that it in no way presumes to be a textbook or a book of fundamentals, but frankly declares itself polemical. Assuming a fundamental groundwork, its primary concern is with the application of Husserl's phenomenology to the problems of perception and particularly to the problems raised by Gestalt. The openly controversial tone of the book is everywhere manifest and constitutes its justification. The work is determined by a freely announced program of ignoring all genetic, and principally physiological, considerations. Throughout emphasis is placed on the significance of experimental data, particularly those published since 1919, the date of the appearance of the first edition. As one might expect from Linke's pioneering studies in movement, a great deal of attention is paid to this subject, with the especial purpose of rebutting Wertheimer. Obviously there is a place in psychology for such a book which, though too frequently unconvincing, warmly and capably presents the arguments for phenomenology.

University of Louisville ELLIS FREEMAN

Das Wesen der Ironie. Eine Strukturanalyse ihrer Erscheinungsformen. By R. JANCKE. Leipzig, Barth, 1929. Pp. vi, 111.

Jancke's study of irony cannot be appreciated without recourse to the methods generally used in phenomenological writings, and especially not without the categories developed in Pfänder's *Psychologie der Gesinnungen*. The author seeks to determine the nature of "irony" by analyzing the various forms of irony employed in social intercourse, and the relations between irony and such phenomena as humor, sarcasm, cynicism, etc. A "lesser" and a "greater" irony are distinguished. The lesser irony is, for example, always a mixture of "manifest" and "covered-up" irony. In case the ironical attitude is permanently assumed by a person, we have the greater irony. The greater irony is a *Lebensgefühl*.

It is not possible here to evaluate critically the phenomenological approach to the study of attitudes, feelings, and emotions in general. The value of Jancke's book is inextricably linked up with the results of such a critique. It may be said at least that, no matter how questionable some of the findings of the phenomenologists are, studies of this kind bring home to us the complexity of emotional states, a complexity which is often overlooked in experimental research for the sake of obtaining "measures of" emotions.

Behavior Research Fund HEINRICH KLÜVER

Essai sur la logique de l'invention dans les sciences. By JAQUES PICARD. Paris, Félix Alcan, 1928. Pp. 288.

The present essay by M. Picard, which is one of a series in the *Bibliotheque de Philosophie Contemporaire*, attempts to show that logic, far from being merely the science of proof, is even more the science of discovery. The author argues that the methods of logic have combined with precise experimental methods in the discovery of new facts and in the progress of science. A wealth of evidence is adduced from the history of astronomy, physics, chemistry, medicine and from mechanical invention to show that scientific method embraces both induction and deduction regarded from the standpoint of discovery rather than from that of proof.

Logicians and philosophers since Bacon have regarded induction as the method peculiarly adapted for scientific discovery, but have treated the deductive method as the method of proof. Picard finds abundant evidence in the history of the sciences for opposing this view. Many discoveries in astronomy, physics, and chemistry have resulted from deductions based on known facts and laws or on assumptions which have been suggested. It was in large part by deduction that Galileo was led to the discovery of the laws of falling bodies, and Maxwell to the discovery of the electromagnetic wave. "A mathematical law is of greater importance than an ordinary discovery," wrote Arago, "for it is itself a source of discovery."

The essay is an elementary and concrete study of the logical method as illustrated in the history and progress of science and invention rather than a formal and abstract treatment of the laws of thinking. A bibliography of seventy-two titles is appended.

University of Montana FRANKLIN O. SMITH

A Point Scale of Performance Tests: *Vol. I, Clinical Manual.* By GRACE ARTHUR. New York, The Commonwealth Fund, Division of Publications, 1930. Pp. ix, 82.

This manual has been prepared to meet the need for a non-verbal scale of individual tests, comparable, in its field, to the Binet scale for children suffering no school training or language handicaps. The present scale of performance tests seeks to improve upon previous performance tests by adequate standardization against a large sampling of representative public school children. The inference from the discussion (comparing I.Q. of testees with Binet I.Q.) is that this test is the same sort of 'general intelligence' test as the Binet scale, except, perhaps, in cases having special forms of "non-verbal abilities." It should also be noted that in earlier preliminary reports of this scale it was called An Absolute Intelligence Scale.

The manual contains two forms of the performance scale. Form I consists of the following tests: Knox Cube, Seguin Form Board, Pintner Two-Figure Form Board, Knox Casuist Form Board, Pintner Manikin, Knox and Kempf Feature-Profile, Healy Mare and Foal (modified), Healy Picture Completion I, Porteus Maze, and The Kohs Block Design; and Form II, of Knox Cube (reversed), Seguin Form Board, Gwyn Triangle, Paterson Five-Figure Form Board, Glueck Ship Picture Form Board, Healy Picture Completion II, Porteus Mental Maze (number at left of subject), and Kohs Block Design (designs inverted). Form I was standardized on responses by 1,100 children and Form II on responses by 535 of the same children about a year later. "For fifty percent of the cases tested on Form I, the I.Q. falls within five points of the I.Q. obtained on either the Kuhlmann-Binet or the Stanford-Binet scale" (p. 4). The P.E. of measurement between Forms I and II ranges from 4.52 I.Q. points for the 6-year-old children to 8.45 for the 14-year-olds. For the highest age-group (16 years) it is 7.97 points. Careful descriptions of the tests and materials and directions for use are contained in the *Manual.*

While the scale is designed mainly as a supplement to the Binet tests, particularly as to the rating of children deprived of the usual school and language training, it is also said to be serviceable in cases of children requiring special

diagnostic tests, and as a means of breaking down timidity and of establishing interest and proper test-relationships for the application of the Binet scale.

The present volume is intended for use by adequately trained clinicians. A second volume is promised which will give information on the details of the scale construction and standardization. It supplies age-norms for the point scores covering chronological ages 5.5 through 15.5 years for Form I, and 6.5 through 16.5 for Form II, with some downward and some upward extrapolations printed in italics.

It is known that the reliability of some of the performance tests used is low. One wonders, in the absence of the promised data of Volume II, whether the selections of specific tests are the most happy in some cases. Criticisms on this score, however, are as yet out of place. The discriminative value of each test is measured by a formula involving P.E. of distributions but there is no correction of these P.E. for unreliability. J. P.

Krankhafte Seelenzustände beim Kinde: Grundlagen für das Verständnis intellektueller und sittlicher Regelwidrigkeiten. By O. HERMANN. 3rd. ed. Langensalza, Beyer & Sons, 1930. Pp. xv, 252, 7 pl.

The major portion of this monograph (208 pages) is devoted to an account of various forms of mental impairment resulting from "outer" causes. A second section of 44 pages describes mental abnormalities resulting from "inner" causes, "for the most part of an hereditary nature." The first group includes all forms of mental disorder or defect which have or may be assumed to have a physical basis, such as hydrocephaly, microcephaly, mongolism, cretinism, etc. General mental deficiency of all grades, and special disabilities such as alexia and agraphia, are also included in this group, since Hermann assumes that these are the result of cerebral deficiencies, either general or confined to specific brain-areas. In addition he lists a small group of "mental causes of mental impairment" in which gross bodily accompaniments are lacking.

Under the second head are included lack of self-confidence, oversensitivity, the "psychopathic constitution" etc. Neither the grouping nor the point of view upon which it is based would be accepted by most American psychologists of today.

University of Minnesota FLORENCE L. GOODENOUGH

The Enlargement of Personality: Behavior Patterns and Their Formation. By J. H. DENISON. New York, Charles Scribner's Sons, 1930. Pp. xvii, 340.

The author's thesis may be very briefly, and baldly, stated thus: personality is the behavior-pattern of the individual. This pattern is due to the influences of environment playing upon one's capacities. Included in these influences are: the family; childhood gangs; tribal beliefs (taboos, social standards, ideas of caste); and the individual's concepts of himself (standards, ideals, etc.). One's personality may be changed in some cases by auto-suggestion, in other cases by the influence of religion or other strong motivating factors, and in still others by a carefully planned objective involving standards rigidly adhered to. The method of changing depends upon the individual's receptivity to suggestion. Emotions are important in giving intensity to various behavior patterns.

Stated in such fashion, the text presents very little that is new to the professional psychologist. There is a large amount of anecdotal material giving point

to the above thesis and making it very readable. The lack of specific citation of sources is not necessarily a drawback to an avowedly popular book such as this is.

While there are many minor details and statements with which most psychologists would disagree, taken all in all the book represents a fairly sound popular treatment of the subject. This opinion is given in spite of a prejudice aroused by the publishers' statement: "Here is explained a successfully tested method for becoming the person you would like to be, for attaining happiness, peace, or whatever your *real* objective has been."

University of Pittsburgh — J. H. McFADDEN

Introduction to the Psychology of the Classroom. By CHARLES E. HOLLEY. Boston, D. C. Heath & Co., 1930. Pp. xiii, 257.

This book is obviously of interest to those concerned only casually with applications of psychology in the schoolroom, rather than to those interested in the presentation of a system of scientific facts. It is evident that the author is more educationalist, of a practical sort, than psychologist. In the main the facts and suggested principles upon which the educational applications rely are true, and the interest of the book for the psychologist is found in the varying aptness with which the author shows how the principles apply. As to the so-called instincts he is somewhat too vague and perhaps too confident, and he is a little disregardful of the latest findings respecting sex differences. There are numerous statements made in good faith which many real psychologists would hesitate to make. It seems to the reviewer, moreover, that the author, being an educationalist of the practical sort, is disposed to give accepted fact a turn to suit his practical purposes, which may be not only unwise but also unwarranted.

University of Denver — THOMAS R. GARTH

Social Psychology. By J. J. SMITH. Boston, Richard G. Badger, 1930. Pp. xxv, 468.

This treatise is characterized by the author as "the psychology of social attraction and repulsion," and aims to present the field of personal and social relationships from the point of view of psychology. It outlines a systematic point of view in which the impulses and feelings, organized into sentiments approximately after the manner of Shand and McDougall, are basic. Desirable sentiments may evolve in genetic growth and in the modification of innate tendencies. An understanding of the organization of sentiments reveals their relation to stability of social groups and relations between individuals and groups.

The author's discussion becomes somewhat incoherent at times, due perhaps to the fact that he makes generous use of quotations and citations. Many pages have from four to six of these. All but five of approximately 250 references are books, and no notice is given to contemporary trends in social psychology as revealed in recent journal literature. Although the author states that "psychology must be based on ascertained fact" his arguments are, for the most part, based on opinions from systematic discourses rather than facts discovered by experiment. The reviewer believes that, to be of value to contemporary psychologists, no treatise on social psychology can neglect the objective viewpoint or experimental method.

University of Minnesota — MILES A. TINKER

L'activité psychique chez les animaux: Instinct et intelligence. By L. BRETEGNIER. Paris, Vigot Bros., 1930. Pp. vii, 387.

The reader of this book is taken through a large part of the domain of animal psychology, including its history, its major problems (especially instinctive behavior), and a part of its experimental achievements. The book is richly but carelessly documented with references drawn for the most part from French authors, most of the research done in America being left out of account. Köhler comes in for several pages but other references to German researches are scanty. Some of the older studies of Watson, Thorndike, Morgan, Yerkes and a few other papers by Hunter, Lashley, Jennings, and Loeb are listed in a poorly selected bibliography; but no use has been made of some of these references in the text. Such citations as the "Britisch Journ. of Psych.," R. S. Lashley, J. B. Wahson, and W. M. Weeler, numerous omissions of dates, the meager number of references to studies published after 1915, and dozens of typographical errors do not increase the confidence of a reader in the scholarly quality of the book.

The author is a veterinarian, and has written more in the temper and with the empirical outlook of Romanes than of Washburn or Hempelman. Descriptions of animal behavior from the *Atlantic Monthly* or from *Mercure de France* stand side by side with data from Hunter, Watson, and Thorndike and not a little space is given to Jack London and James Oliver Curwood. The whole account is enriched by the author's experiences from his professional career and by literature from fellow veterinarians.

The author's early statement of program consists of a pointed self-instruction against undue anthropomorphism and of a vote in favor of Lloyd Morgan's canon; but almost with the same sweep of the pen the author argues for a liberal interpretation of animal behavior in terms of human experience; and as soon as the review of the literature gets under way, it is the last injunction rather than the others that becomes dominant.

The first chapter is devoted to a history of animal psychology in which a recital is made of the opinions of ancient and medieval authors. It is followed by a study of the methods that ought to be employed in animal psychology. It is here more than in any other chapter that the author tries to place himself inside of American research; but the attempt is not impressive.

Four chapters, totaling 134 pages, are devoted to tropisms, reflexes, and instinct, one chapter being given to the instinctive behavior of the social insects. The discussion of tropism is hardly more than a review of Loeb and is fair enough in its treatment. Reflexes are considered primarily in their rôle as the foundation of instinct and intelligence. Instincts are described as having a double origin. They represent complex and stabilized reflexes which can be transmitted by heredity, and are descended from acts which were originally reflective and conscious and have become unconscious through repetition. These, too, are said to be heritable. Thus the nature of the descent from intelligence to instinct is indicated; but we are not told how the ascent from instinct to intelligence was accomplished. There is throughout the book the implication that the genetic history of mind is to be read in terms of a progression through tropisms, reflexes, and instincts to intelligence; but this is an assumption that is seriously in need of critical study and possible revision.

The chapter on emotions (26 pages) draws much of its material from Köhler, Romanes, and Darwin, on the one hand, and from general experience, mostly

veterinary, on the other. It does more to set a problem and to throw light upon lacunae in research on animals than it does to indicate progress in understanding this part of animal psychology. The chapter on the intelligence of animals begins with a lengthy account of animal tricks and genius drawn from the tales of sportsmen, veterinarians, and trainers, and it ends with the usual remarks about "clever Hans," the Elberfeld horses, and Köhler on chimpanzees. There is, here, as in most of the other parts of the book, nothing more than a recital of facts and observations with hardly a critical remark on what has been reviewed.

The book closes with a short chapter on language, which contributes almost nothing, and a brief résumé of the mental life of the higher animals as compared with man.

The general conclusions do not represent a summary of the significance and general bearing of the researches mentioned but only a diffuse and somewhat philosophical statement of the beliefs and opinions, not to say prejudices, of the author.

University of Illinois — COLEMAN R. GRIFFITH

The Layman Looks at Doctors. By W. S. and J. T. PIERCE. New York, Harcourt, Brace & Co., 1929. Pp. 251.

This is a narrative of a series of episodes in the life of an only daughter of an elderly physician, an educated and sensitive young woman who suffers a "nervous collapse" shortly after an apparently happy marriage to a devoted lover and husband. The story of her period of mental aberration is clearly and somewhat dramatically developed from a lay point of view by the writers, husband and wife, whose early married life is thus interrupted by the latter's abruptly appearing illness. In her efforts to regain her mental and social equilibrium, Sally is forced through a chain of unhappy and unfortunate experiences with sanitoria, hospitals, and nursing homes, which are graphically portrayed, and which are terminated, according to the account, only with the treatment by a psychoanalyst to whom her final recovery is gratefully ascribed.

There appears from time to time a slight tendency toward projection mechanisms on the part of the writers, who, as laymen, also do not seem to distinguish clearly between the province of the neurologist and that of the psychiatrist, nor to recognize that psychoanalysis, however useful it may prove, has likewise its definite limitations. It is possible that this carefully detailed account of unfruitful medical contacts may, through hasty generalization by the suggestible reader, prejudice him against seeking necessary medical aid, perhaps much to his own disadvantage. On the other hand it should prove a tremendous stimulus to critical self-analysis of motive and achievement on the part of earnest and sincere physicians.

New York City — HAROLD A. PATTERSON

Our New Progress. By J. B. CLARK. New York, G. P. Putnam's Sons, 1930. Pp. 128.

This is not a scientific book. It is a theoretical consideration of what real wealth is, and devotes especial attention to fallacies and dangers connected with charity in its various aspects. Transmission of acquired characters is naïvely assumed and consciousness is regarded as a causal factor. J. P.

The First Year of Life. By CHARLOTTE BÜHLER, and others. Translated by Pearl Greenberg and Rowena Ripin, with preface by translators. New York, John Day, 1930. Pp. x, 281.

Part I appeared originally as Bühler and Hetzer's *Inventar der Verhaltungsweisen der ersten Lebensjahres.* Part II is Hetzer and Wolf's *Babytests* and Hetzer and Koller's *Eine Testreihe fur das zweite Lehensjahr,* with directions for giving the tests, method of scoring, and validity. Part I is a description of the continuous observation over a period of two months of the activities of 69 infants under one year of age, in an institution for children in Vienna. Sixty percent were institution children and 40% were observed in their homes or were brought to the institution by the mother. The observations were made under normal conditions, the setting uncontrolled in every way, the observer noting every change in the child's behavior. The author admits that she and her assistant soon came to the limit of language to represent all movements and vocalization. They then made use of a finer method of notation, but unfortunately do not reveal its nature. The aim was to obtain a composite picture of normal behavior —uninitiated by any artificial stimuli—a picture to serve as a standard for average development. We are told nothing, however, of the amount of handling each child received, how many individuals came in contact with him, and so on. The author regards experiments which utilize artificial stimuli as lacking in scientific value and therefore found it unnecessary to make any attempt to evaluate qualitatively or quantitatively the stimulating conditions surrounding her subjects. In reading the book we must distinguish between what the child does under a so-called normal environment and what he is capable of doing under given stimulus conditions. For example, the author finds that the infant makes no movements of defense until the fourth month. Yet it is well known that under certain conditions the infant as early as two weeks makes definite coördinated movements of defense. An adequate picture of normal development should include not only what a child does in a restricted environment but also what he is capable of in response to the varied stimuli he must more and more be prepared to meet as he develops. Following Part I is an appendix containing a schedule of infant behavior, month by month. The inventory lists the observable behavior of the child at a given age, with amplifying descriptions. The interpretative element is prominent throughout, names such as 'fright,' 'joy,' and 'ill-humor' being given to groups of reactions made in response to stimuli known to the observer. The book is an excellent example of the diary method, valuable to those interested in a description of infant behavior.

Chicago, Illinois IRENE CASE SHERMAN

The American Road to Culture. By GEORGE S. COUNTS. New York, The John Day Co., 1930. Pp. xiii, 194.

Contending that the American road to culture is none other than the American educational ladder, Counts here gives us a running critique of our entire educational system. His treatment closely approximates his objective of looking "at America through the eyes of a foreign observer"; it is admirably unprejudiced, fair, and happily free from any trace of cheap superficiality.

DePauw University STEPHEN M. COREY